

# Antiquity

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## Editorial Notes

THE Prehistoric Society of Great Britain has followed the good custom of choosing some particular aspect of archaeology for discussion at its meetings, which are spread out over three days. This gives unity to its proceedings and ensures that the discussions which ensue shall be both lively and well informed, because the meetings are attended by leading authorities on the subject selected, both native and foreign. During the last decade the Spring Conferences have been held in London in the pleasant quarters of the London University Institute of Archaeology in Regent's Park. The last meeting was held from April 15th to 17th, and the subject was 'The Reliability of Archaeological Evidence'.

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The Conference began with an address by Professor Hawkes of Oxford on 'Believing in Archaeology', and the other papers dealt with evidence from pleistocene geology and soil science, the stratigraphy of later sites (chiefly Roman and Medieval London), the statistical method in palaeolithic archaeology (in France), the reliability of excavation reports, and petrological evidence. Professor Hawkes then gave an account of the British contribution to *Inventaria Archaeologica*, a project whose first publications have already been noticed here (ANTIQUITY, XXVIII, 176). Miss M. Smith discussed 'The Limitations of Inference in Archaeology' in a stimulating paper to which we shall refer again later.

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Palaeolithic archaeology is a French speciality, and it was therefore quite natural that the latest device for tabulating the types of implements should be described by its inventor, Monsieur Bordes, Chargé de Recherches at the Centre National de Recherches Scientifiques, Paris. The method is necessarily laborious; the results are expressed by means of graphs of frequency which enable the vast numbers of implements yielded by occupation sites to be handled in a way that is otherwise difficult because of their very abundance. M. Bordes showed that the personal factor in typing the implements is virtually eliminated; a test showed that the graphs of two independent examinations were practically identical. His researches on these lines are being published in *L'Anthropologie*. It will be interesting to see the method applied to other regions, periods and raw materials; there seems no reason why it should not be equally useful in the mesolithic period and in the Stone Age of America. Could it also be applied to such things as the conventional designs on rocks and pots?

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Dr Oakley described some of the problems of unravelling the sequence of river terraces and of glaciations, whose solution is the key to man's cultural evolution in Britain and in many other parts of the world ; and Dr Cornwall showed how the new technique of soil analysis can help the excavator—and how in turn the soil analyst is dependent on the excavator for providing him with carefully selected material.

Mr Grimes has been allotted the task of probing into the foundations of London. His chief discovery, in popular opinion, was the temple of Mithras and its fine sculpture ; but whereas this was the outcome of a lucky accident, his discovery of the Roman fort which explained the remarkable right-angled turn in London's north wall, was due to his meticulously careful methods of excavation, and is from a historical point of view the more important of the two. Some idea of the complications of the stratification was given by slides showing how Roman and Medieval pits and post-holes bored into the strata, reducing them to the state of a Gruyère cheese. Excavating here is what Sir Mortimer Wheeler calls ' watchmakers' work '.

Excavation reports are the foundations of archaeological knowledge ; and at a Conference devoted to the examination of foundations it was most important to discuss their strength or weakness. Mr Atkinson's contribution emphasized the fact that every excavator's report must necessarily be a selection of the observed facts, however complete the record and however careful and objective his methods. Everyone, both in excavation and in other forms of activity such as field-work, tends to see only what he expects to see or is looking for ; and there is also a risk of ignoring or underestimating the value of what one is not interested in. He cited the discovery of the axes carved on Stonehenge which he found because his interest was, for the moment, concentrated on carvings of a much later date. The evidence of excavation reports is secondary, not primary ; excavation must always destroy evidence which cannot, as in other branches of science, be consulted again ; hence there is an even greater responsibility to observe and record conscientiously. We would add that the observation of negative evidence—what is absent—is of great importance ; and it is because only the excavator himself can affirm this from his presence at the excavation that his own report is so much more valuable than can be a report written up later from his records by someone who was not present.

Miss Smith was concerned with the Limitations of Archaeological Inference, which she described as of two kinds. Inferences of the first kind are legitimate and trustworthy, as when trade relations are inferred from the distribution-map of objects (e.g. gold lunulae) ; those of the second are not. She illustrated her argument by citing an inference that excavators have made—that in a hut village the biggest hut must have belonged to the chief ; it might also have been a shrine or a place of assembly. (As an example of similar inferences she might have quoted those drawn from the ' Royal Tombs' at Ur). Even more perilous was the inference of political facts from type-distributions.

It is always stimulating to listen to criticism of accepted beliefs, provided of course there is, as here, complete freedom of discussion ; a stringent criticism tightens up our mental processes and is a cure for loose thinking, even if we do not wholly assent to it.



## EDITORIAL NOTES

Miss Smith has a good case, but one felt that the other side might be able to put up quite a good defence. What, for instance, have anthropologists to say on the 'big hut' problem? Have they indeed ever recorded any observations pertinent to it? It is the fashion nowadays to decry anthropological parallels; we think it a silly fashion, though admittedly the comparisons must be made warily and whenever possible within the same restricted culture-circle.

Some of the most dangerous inferences, we would add, are those made about primitive religious practices. Everyone knows that when an excavator or museum curator is puzzled he can always explain a useless-looking object either as intended for some religious purpose or as 'horse-trappings'. An hypothetical example from modern practice was cited in the discussion. In the semi-desert island of Fuerteventura, the arable fields are separated by very low walls only, that any sheep or goat can easily cross. Not all are under crops at all times, so that the little herds of these animals can often browse on them with impunity. When a crop has been sown, in order to indicate the fact to the herdsmen (usually quite young children), small piles of single flat stones are set at regular intervals along the walls. They then know that they must not let the animals stray into these fields. How difficult it would be for an excavator to explain these little piles! And how tempting here would be the resort to a 'religious (or magical) purpose'.

Excavations at Silchester, Hampshire, are now being arranged for September 1955. Whereas much is known of the buildings, plan, and defences of the Roman cantonal capital, little is known of its foundation and the pre-Roman settlement which preceded it. The site is *Calleva Atrebatum*, the capital of the British tribe of the Atrebatas, whose dynasty of Belgic princes can be traced back to Commius, ally and later enemy of Julius Caesar. Silchester is known to have been occupied at least as early as A.D. 10 because there are Ancient British coins of that date bearing the name of the city. There is besides a considerable amount of pre-Roman material in the great Silchester Collection at Reading Museum; but neither this nor the coins can be referred to any structures or levels on the site itself.

Recently however in an aerial survey, Dr St. Joseph discovered a buried defensive system which when tested in a preliminary excavation last year, proved to be of considerable dimensions and of earlier date than any of the other three defences. There seemed in fact good hope that further work would be able to prove that the new defences were those of the long sought Belgic settlement. The enclosure seems to be nearly as large as the later Roman city (100 acres) and the new defences are polygonal with inturned entrances.

In the work which is planned for September, fresh sections will be cut across the new defences in an attempt to date them more conclusively, and search will be made for contemporary occupation inside. A Committee has been formed to superintend the work. Its Chairman is Mrs M. A. Cotton, who excavated at Silchester in 1938-9; the Treasurer is Colonel C. N. Rivers-Moore of Remenham Hill House, Henley, Berks., to whom contributions may be sent; and the Secretary and Director of excavations is Mr George C. Boon of Reading Museum. We wish them success in their undertaking.

# The Quern-quarries of Mayen in the Eifel

by the EDITOR in association with J. RÖDER and others

EVERY prehistorian has heard of the famous volcanic rock of the Eifel from which querns were made in prehistoric and later times. The source from which the querns came has several names, and we read of Andernach, Niedermendig and Mayen lava. Actually, Andernach is merely the Rhineland town nearest to the quarries; there is no lava there. Niedermendig has lava quarries, but they did not begin to be worked until the Middle Ages, and then the output was only about one third of that of Mayen. Since however the output of Mayen passed through Niedermendig on its way to the Rhine, the latter name got attached, and we in Britain usually write of 'Niedermendig lava'. But all prehistoric and Roman querns came, not from there but from the Mayen quarries, which are still being worked. During a visit to Germany in July 1953, I was conducted round them by Dr J. Röder of Koblenz. The following account, though made possible by that visit, is not an original composition but is derived from an authoritative article<sup>1</sup> by Dr Röder and others, which has kindly been abstracted for me in English by Mr J. D. van der Waals of Amsterdam. I wish also to thank Dr Röder for the loan of illustrations, especially the blocks for FIGS. 1 and 3.

It comes as a surprise to learn that the first recognition of the archaeological importance of these quarries was as recent as 1914, when Peter Hörter published his first article in *Mannus*. In 1947 a working group formed to investigate the history of the quarrying industry at both Mayen and Niedermendig. The article here summarized is its first report; the second will deal with the later (medieval) phases, and with Niedermendig.

The lava-field has the name of Bellerberg and lies between the places of Mayen, Ettringen and Kottenheim, with an area of about six square kilometres. Two streams of lava burst out from the crater, one flowing southwards to the Mayen quarries, the other northwards to those of Kottenheim. The stone is a soft basalt (resistance 900–1500 Kg/cm<sup>2</sup>), and is therefore easy to quarry, while its porosity prevents the grinding surface from ever becoming smooth.

The lava was first used extensively for querns during the Hallstatt period (1200–400 B.C.). Before this, sandstone was often used even in this region; a late Neolithic sandstone quern was found at Kollig. It has been possible to establish a succession of types of quern; the evidence of date is based not upon the uncertain stratigraphy of the quarries but upon quern-finds in dated settlements. The primary division is of course that between saddle- and rotary-querns<sup>2</sup>, and here it comes during the La Tène period (400–100 B.C.) doubtless under the influence of Southern contacts. Saddle-querns are, of course, used with a much smaller stone, like a large soap-cake, that is rubbed up and down over their surface.

<sup>1</sup> *Die Geschichte der Basalt Lava industrie von Mayen und Niedermendig*, by F. Hörter, F. X. Michels and J. Röder. Part 1: 'Vor und Frühgeschichte, Jahrbuch für Geschichte und Kultur des Mittelrheins und seiner Nachbargebiete': 2–3 Jahrg., 1950–1, pp. 1–32. As references are given there they are omitted here.

<sup>2</sup> For which see Dr Cecil Curwen's article in *ANTIQUITY*, XI, 133 ff.



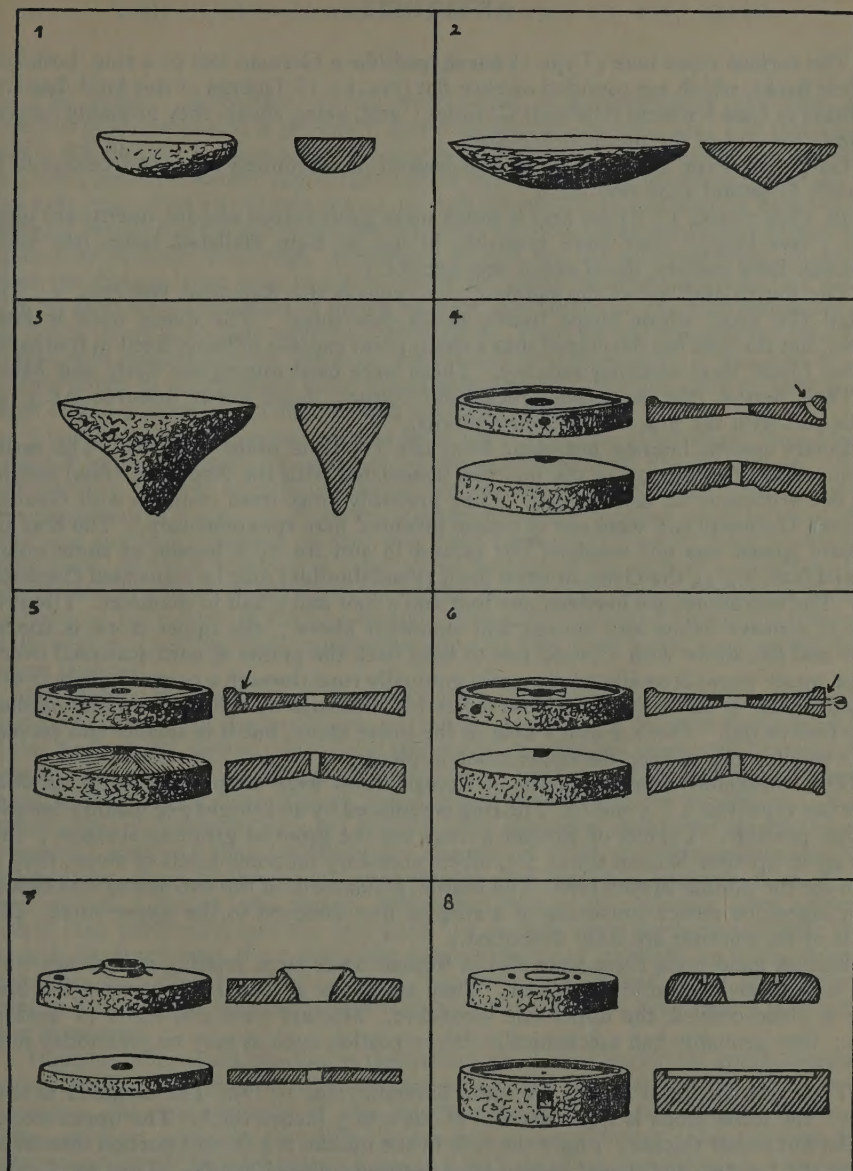


FIG. 1. MAYEN QUERN-TYPES (Röder, fig. 2)

1. Neolithic to Middle Hallstatt
2. Late Hallstatt (600-400 B.C.)
3. Napoleon hat (Early and Middle La Tène)
4. Oscillating rotary quern (from La Tène onwards)
- 5 and 6. Completely rotating quern (Roman period)
7. Early Medieval
8. About A.D. 1000

The earliest types here (Type 1) are shaped like a German loaf or a slug, both lying on their backs, which are rounded or even flat (FIG. 1; 1). Querns of this kind date from Neolithic to Late Urnfield (Hallstatt C) times; and, being cheap, they probably survived in use to the end of the prehistoric period.

Querns of Type 2 have a slight keel instead of a rounded base, and belong to the Hallstatt C period (800-600 B.C.).

In Type 3 (FIG. 1; 2) the keel is much more pronounced and the querns are bigger (2 to 3 feet long); they were probably in use in Late Hallstatt times (the earlier Hunsrück-Eifel culture, dated about 600-400 B.C.).

The fourth and last of the saddle-quern types is the Napoleon Hat (FIG. 1; 3) or Cocked Hat type, whose shape hardly needs describing. The quern itself is rather shorter, but the keel has developed into a sharp point capable of being fixed in (perhaps) a wooden block, thus ensuring stability. These were used during the Early and Middle La Tène period (the later Hunsrück-Eifel culture, dated about 400-100 B.C.), and overlapped with the first use of rotary querns.

Rotary querns became prevalent from late La Tène times onwards. The earliest examples have been found in the quarries (associated with the Napoleon Hats) and in a La Tène settlement at Kottenheim. They probably arose from contacts with Gaul and Southern Germany and were not of course invented here spontaneously. The first kind of rotary quern was not revolved but moved to and fro by a handle of some organic material (FIG. 1; 4), the German word for it (*Pendelmühle*) may be translated Oscillating type. The two stones are between one foot and a foot and a half in diameter. The lower stone is concave below and convex and smoother above; the upper stone is concave below and flat above with a raised rim to hold back the grains of corn scattered over it. As the upper stone is oscillated the grain gradually runs through a central hole in it to be ground, while at the same time it serves as ball-bearings on which the stone oscillates easily (PLATE III). There is also a hole in the lower stone, but it is smaller and serves to hold a wooden peg to keep the upper stone in place.

The completely rotating querns of Roman times were simply improved variations of the last type (FIG. 1; 5 and 6). The ring is replaced by an upright peg making complete rotation possible. Typical of Roman querns are the grooved grinding surfaces; these were given up after Roman times, for, albeit necessary for some kinds of stone, they are not so for the porous Mayen lava. The mutual adjustment of the two stones was ensured by an ingenious device consisting of a strip of iron fastened to the upper stone. (The details of its working are fully described.)

Besides hand-mills there were also in Roman times large rotating mill-stones which must have been mechanically driven. They are about  $2\frac{1}{2}$  feet in diameter; the lower stone is plano-convex, the upper one biconcave. Mortars were also made to husk the grain; they probably had mechanically driven pestles, such as may be seen today in the East.

The early Medieval querns are quite different (FIG. 1; 7). The diameter is about 2 feet; the lower stone is quite flat and about 2 to 3 inches thick. The upper stone is also flat but rather thicker; round the hole in the middle is a thicker portion intended to receive the hopper, and it was turned by a vertically placed handle. They were clearly designed for mounting in a wooden framework, as are the comparable Galician mills at the present day. The millstones of about A.D. 1000 (FIG. 1; 8) have great lower stones with a raised rim within which revolves the (smaller) upper stone; the flour escapes through a hole in the rim of the lower stone. Most of these millstones are large (diam. about  $2\frac{1}{2}$  feet), but hand-mills (querns) also occur.



## THE QUERN-QUARRIES OF MAYEN IN THE EIFEL

### QUARRYING

The present appearance of the quarry-field is an impressive reminder of the labour of generations and is probably unique in Europe (PLATE I, A). The traveller who visits it will find the existing open-cast quarries with their modern equipment side by side with the quarries of earlier times, the work of the Old Men, as the miners now call them, who stole from them all the best stone! He will see the rooms and sloping galleries of the medieval period and the more recently exploited but now abandoned underground mines, exposed by modern quarrying. Elsewhere he will see the quarried walls immediately below the surface layer, next to places where the basalt has been quarried at a depth of 15 to 25 feet, with huge piles of debris on top. Elsewhere he will find people removing this debris so as to re-start quarrying beneath it. Then he will see that the rock-face is covered with ancient grooves, traces of wedges and other markings. It took half a century of observation to interpret all these traces of millennia of mining, and to form a visual image of the quarrying-practice of each successive period.

That the Mayen stone was worked from Early Neolithic (Danubian) times onwards is proved by the discovery of exported querns associated with datable objects. But no quarries have hitherto been found dating before Late Hallstatt times when there was a great expansion of trade. Such earlier quarries may have escaped notice because of their small size and insignificance, or they may have been obliterated by later workings. It would seem probable however in view of the flatness of the earliest querns that before Late Hallstatt times all needs were met by the loose surface material which is abundant, especially on the margin of the lava-stream. A recently excavated Urnfield settlement on the Eicherkönnchen produced querns that seem to have been made, even at that date, of surface material.

In Middle Hallstatt times (about 800-600 B.C.) regular quarrying began on a broad front along the edge of the lava-flows, where the later volcanic deposits above are thinner. In the Kottenheim region particularly this early quarrying is evidenced by huge undisturbed piles of debris lying in terrace-formation. These early quarriers removed only the upper layers, leaving quantities of useful basalt for the present generation. As they worked forward, however, they left behind them masses of quarry-refuse resting on the unquarried rock and thus rendering the remains of their activities inaccessible to the investigator; these accumulations grew bigger and bigger in proportion as the overlying volcanic (pumice ash) deposit, which had first to be removed, grew thicker the further they penetrated into the lava-flow. It is only when modern quarriers looking for rock have removed these overlying accumulations of rubbish, or when these have themselves been quarried away for stone-fragments and for road-metal, that the ancient working-floor can be examined—and then only for the short time before it is quarried away once more.

However, these rubbish-dumps themselves are just as important archaeologically as the old surface beneath, for they contain many half finished and broken or cracked specimens, the manufacture of the querns having been carried on on the spot. These are most useful in revealing the process of manufacture. The wasters form a high proportion of the total output because a high standard was enforced. In 1937 the Landesmuseum of Bonn made and recorded a cross-section in a rubbish-mound in the Kottenheim region and in an excavation of a La Tène settlement in the same region, fragments of Napoleon Hats and of oscillating querns are found together in association<sup>3</sup>.

<sup>3</sup> See *Bonner Jahrbücher*, 145, 1940, pp. 260-2; 146, 1941, pp. 395-403.

As the technique of quarrying improved, and particularly by reason of a new method of winning the stone, new quern-types—boatshaped, and the Napoleon Hats—made their appearance. The older flattish types had corresponded with the naturally detached fragments (found in the upper layers and on the surface) which are dish-shaped; and in a similar fashion the shape assumed by the artificially detached fragments became a decisive factor in shaping the new types of quern—together of course with others relating to use and handling. In fact these artificially detached fragments often take the form of hybrids between Napoleon Hats and boat-shaped querns.

The quarrymen's chief tool down to well into the La Tène period was a hammer-stone of hard basalt, grooved doubtless for mounting in a double sling and varying in weight between two and thirty-five pounds. Those of medium weight will have served to cleave the basalt columns in such a way as to get raw material of the right shape. By careful hammering it was possible to prevent splitting off too thin a flake. The hammering created grooves in the rock, broad and slightly rounded by reason of the shape of the hammer used (PLATE I, B). After the clearing process the block was detached by a single blow from one of the heaviest hammers. Many of the hammers found are much worn and have evidently been resharpened more than once. There are also found stone balls of hard basalt (diam. 2 to 4 inches) which may have been used for the final smoothing of the grinding-surface of the querns after these had been brought to the desired shape with the lighter hammers.

It had been observed that the later pre-Roman querns (Napoleon Hats) have on their surface not only the oval marks made by stone hammers but also pointed incisions. When the rubbish-dumps were removed there were revealed pre-Roman cleavage-grooves which were narrow and sharp and much more deeply cut than those made with stone hammers. Finally the actual tools with which, as had already been postulated, these pointed incisions and cleavage-grooves were made, namely, two double-pointed iron hammers which, unlike the Roman and later hammers, fit perfectly into the cleavage-grooves in question. With the hammers was found a chisel that had obviously been used to cut the holes in the Oscillating querns. Thus it became evident that iron tools were introduced here during the La Tène period for quarry work; and for a long time hammers of stone and iron co-existed.

With the introduction of iron hammers the quarrying technique improved considerably. A firmer control was established over the cleaving direction of the rock, thanks to the deep narrow grooves, so that a more economical exploitation was achieved and the shape of the pieces split off could be determined beforehand to suit the demand. Even so the final blow had still to be given with a heavy hammer, and this practice lasted until about the end of the prehistoric period when wedges began to be used for splitting off the blocks.

If we assume that pre-Roman quarrying extended along a front of about 5000 yards and penetrated about 50 yards into the lava-flow, and that the average thickness of the rock quarried was 5 yards, it follows that a total amount of about a million and a quarter cubic yards must have been removed; this is certainly not an underestimate.

In Roman times the output constantly rose as trade expanded; moreover Mayen stone was also used for building (e.g. the Roman bridge at Trier, still in use) and occasionally by sculptors as well. Now and subsequently, though the descendants of the old quarrymen may have continued to work, more advanced methods were introduced from Gaul and Italy. Under the Romans cleavage-grooves were no longer made, but instead holes were cut with a chisel at intervals of 3 or 4 inches, to hold wedges of a width of 2 inches to 3 inches (PLATE IV). A row of such wedges was actually found *in situ*, jammed





FIG. 2. THE MAYEN QUERN-QUARRIES

1. Prehistoric period. 2. Roman. 3. Early Medieval. 4. Medieval and modern underground workings

Scale, 1:25,000 (Röder, plate 1)





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tight with wooden slithers just as are the iron plates of today, for the firmer the wedges the quicker the cleavage. To achieve this a tapering hammer is used, identical with that in use today; the wedges are tapped one after the other until the stone starts cleaving, when a final blow on one of the wedges suffices to detach the block.

Besides tapering hammers and wedges the Roman iron tools included chisels as before, which were also probably used to make the grooves on the grinding surface, and three other types of hammers: (1) short, thick and wedge-shaped (2) somewhat longer and slightly curved (3) a double-edged hammer still in use. All three have broad edges, and were used to give the blocks their final shape and finish, leaving on them the long sharp marks characteristic of Roman work.

When the Franks replaced the Romans, work in the quarries was little if at all interrupted, but the market shrank, for building in Mayen stone was only resumed on a small scale in the Carolingian and Ottonian periods (9th and 10th centuries). The working method too changed. It is noticeable how all the basalt columns have been quarried away to the same level, leaving an almost horizontal flat surface. The most important tool used was a hammer with a single edge more than half an inch broad, the other half consisting of a long pointed pick, slightly curved. This and the other tools were used for finishing off the millstones and for making the holes, leaving typical short incisions.

ADDENDUM. Since the above-mentioned article appeared, entirely new results have been obtained with regard to the ownership of the Mayen pits in prehistoric and protohistoric times. Though the mining zones of the individual pits are arranged around the fields of basalt like the year-rings of trees, it is evident that the workings did not proceed symmetrically (evenly) from all sides towards the centre of the basalt streams. It is true that during successive periods mining was done in such a way that the new workings joined the older ones; it is, however, also evident that the whole area of pits was parcelled out into individual lots. As far as there is unambiguous evidence, we have in prehistoric times pits of 8 or 10 square metres. In Roman times, obviously for reasons of taxation, the whole area was surveyed and there appear during this time long narrow strips of workings lying obliquely in the Roman working zone, and this system survived into early medieval times. Here this system was discovered earlier and it is mentioned in the article quoted above. When such a lot was being worked, the workings were pushed to the edge of the individual property (lot) and the hole which resulted during the working was afterwards filled in with rubble. The neighbouring owner, when working his lot, left the rock intact at his edge also in order that the rubble of the neighbouring lot should not fall into his pit. Thus the old edges of the workings of the individual owners have survived quite often to a considerable extent. The modern workings which deal with the lower layers quite often uncover these old quarries and their edges. But frequently in pre-modern times the old border-zones between the rubble have been worked also so that we have strips of medieval workings between purely prehistoric and Roman workings. After this had been discovered, the surveying started and we have already mapped considerable parts, especially of the Roman and early medieval pit-system. The first maps will be published in the near future.

### MANUFACTURE

In prehistoric times the raw material was quite certainly worked up in the quarries. In the Kottenheim area the following observations were made during the removal of a rubbish dump. At the bottom were the partly demolished basalt columns (D in FIG. 3) left behind as the quarrying advanced. Several cleavage-grooves could be seen delimiting Napoleon Hats which were left undetached. Above the stumps of the columns was a

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layer of reddish loam (c) representing the old surface soil lying above the quarried basalt. Intermingled with it are patches of stone chippings probably derived from the first roughing out of the querns, before these were taken for the final dressing to a working-place further back. The top layer (B) consisted of rubbish with fewer stones in it, except at two points on the right and left where there is an intensive accumulation of stones (A) dated by the sherds to the Roman and Medieval periods. Layer B itself, however, contained only prehistoric sherds of the Hunsrück-Eifel culture (1 and 3) which dated the working floors 1 and 2 to the date Hallstatt and Middle La Tène periods. Floor 1 only could be completely investigated. Over an area of about five square yards was a deposit about 10 inches thick consisting of stone-chips, rubbish and earth. On the right it was surrounded by a stone wall between four and five feet in height and leaning slightly backwards, which was obviously intended to prevent the rubbish heaped behind it from slipping down. On this platform were found three more or less finished Napoleon Hats,

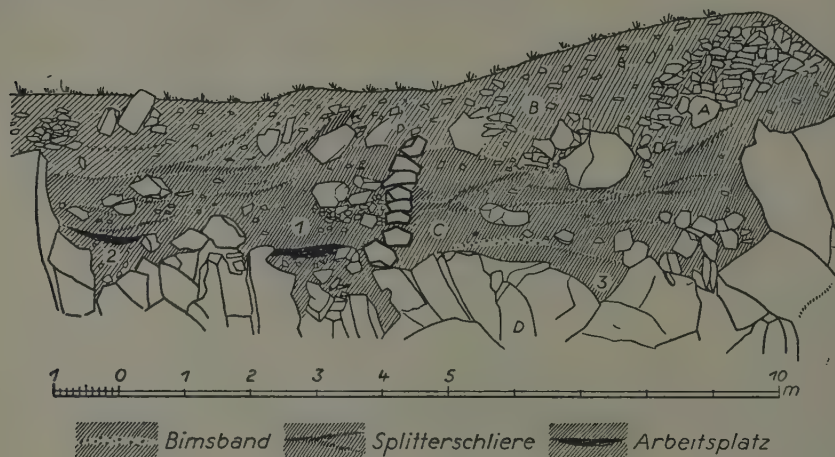


FIG. 3. SECTION OF A RUBBISH DUMP AND WORKING FLOOR AT KOTTENHEIM showing pumice layers, splinter-band and working floor (Röder, fig. 24)

together with a hammer and raw materials. As the working-floor had to be close to the quarrymen it was moved forward from time to time and the old floors were buried under the rubbish-dumps which also advanced with the quarrymen.

From the Roman period onwards the working up of the raw material was removed to the settlements. The half-finished or broken querns so common in the prehistoric dumps are then almost absent. Moreover in Mayen itself houses of the Roman period have been found where floors consist of thick layers of stone chips.

In Haithabu in Schleswig half-finished products of the Mayen quarries were found, proving that such were articles of commerce in early historic times.

### THE STONE HAMMERS

The very hard basalt of which the stone hammers and balls were made is resistant to a pressure of 3300/3600 kg/cm<sup>2</sup>. Petrographic examination shows that some of the stone hammers in the Mayen museum came from the Hochsimmer lava-flow. No



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prehistoric quarries have been discovered there ; they would probably have been on a modest scale and may well have been destroyed by subsequent quarrying or hidden beneath its rubbish-dumps. Other hammers petrographically examined seemed to have come from the Lorenzfelsen on the eastern shore of the Laacher See. There the end of the lava-flow takes the form of a wall of basalt about 300 yards long, with a large gap at one point. At its northern end particularly are large rubbish-dumps lying at the foot of the cliff ; these originally extended to the lake itself, but since then the level of the water has twice receded. The basalt is very dense and must have almost the same resistance to pressure as the Hochsimmer lava. Unfinished and broken stone hammers and balls have been found in the rubbish regularly for a long time past. A preliminary trial trench was dug in the rubbish at the foot of the cliff and brought to light remains of charcoal and a Late Hallstatt sherd. Later, and recently, a more extensive but still only provisional investigation was undertaken, directed by Dr Röder. The first trench was extended in every direction, revealing what seemed to be the base of the quarried stone. The dark soil above it suggested a high charcoal content. In the rubbish above were found balls and stones which may have been intended for making into hammers. The rubbish-dumps were thoroughly examined, but because of the tendency to slip only shallow holes could be dug. There seemed to be a mixture of rubbish due to quarrying and that caused by weathering. Unfinished hammers of every dimension were present. No working-floor was found but a large flat stone (probably not in its original position) had many hollows on it and looked as if it might have been used as an anvil.

How, it may be asked, did the prehistoric people manage to quarry this rock without tools of the same hardness ? The charcoal found and the unnatural crackled weathering of parts of the rock surface suggested the use of fire—a hypothesis that could easily be tested, and was. A big fire was lit against the broad side of an isolated boulder, and kept alight for five hours. Almost at once after it got going small stone-chips started to fly off the rock, travelling for some distance, and small cracks widened. After the fire was removed water was poured over the rock, causing great clouds of steam to rise and the rock to split. Only a few pieces became immediately detached, but shell-shaped bits could now be loosened quite easily to a depth of about a foot. Some bits split up into many small pieces and long slithers, just like those found in the old rubbish-dumps. The big bits were sometimes roughly the shape of a hammer or a ball. Prehistoric men certainly knew how to lay a fire so as to produce the best results, and it must be remembered that they had the added advantage of traditional lore. Rain and frost will have assisted the work. Whole columns of basalt were detached from the cliff.

### TRADE

Our knowledge of the essential features of the quarrying and manufacture of querns and millstones is quite extensive : but, although of the greatest importance for prehistory, the extent and range of the trade in them is still conjectural. Mayen querns are recorded in the Neolithic period from the Belgian Omalian (the Belgian Danubian culture), and in the British Bronze Age from the Sanctuary on Overton Hill, a stone circle excavated by the Cunningtons. According to an unconfirmed report received at the Volcano Museum of Niedermendig, Mayen basalt of prehistoric age has been found in the Shetlands. The earliest markets probably extended over north-west Europe, southern England, the Low Countries, and north-west Germany except the Trier district and elsewhere where good local stone was available. In the Trier district sandstone was in vogue till Late Hallstatt times. But there the situation is obscure even in later times, as it is for all periods in Luxembourg and Eastern France, where basalt products abound. Some of the later ones

may have come from the Volvic lava of Auvergne which is macroscopically indistinguishable from the Bellerberg lava<sup>4</sup>. To the south in the Nahe region and in the Maine and adjacent Rhineland, the local sandstone and porphyry resisted the competition of basalt down to the Hallstatt period. But from then onwards and increasingly during the La Tène period, Mayen basalt emerged victorious, penetrating even into Switzerland. (Even then, however, local quern-factories did not disappear altogether). Eastwards during this period the trade reached the Vogelsberg region. In Roman times Mayen products entered the Neckar region and that of the Upper Danube, but there they might be confused with products of Hungarian basalt. In early medieval times the trade was extended to Scandinavian countries. As has already been stated, there was then a trade also in half-finished products. The stone hammers and balls were also traded, but not beyond the Neuwieder basin.

## Important New Books and Articles

*The inclusion of a book in this list does not preclude its subsequent review.*

EXCAVATIONS AT JERICHO, 1954, by KATHLEEN M. KENYON. *Palestine Exploration Fund Quarterly*, May-Oct., 1954, 45-63.

SOIL SCIENCE AND ARCHAEOLOGY, with illustrations from some British Bronze Age monuments, by I. W. CORNWALL. *Proc. Preh. Soc.*, Dec. 1954, 129-47.

THE STANWICK FORTIFICATIONS, by SIR MORTIMER WHEELER. Bernard Quaritch (for the Society of Antiquaries of London), 21s.

THE VATICAN EXCAVATIONS, and the cult of St. Peter, by HJALMAR TORP (Oslo): *Acta Archaeologica*, xxiv, 1953, 27-66. [A scholarly analysis of a bad excavation].

THE HANGING BOWL, a liturgical and domestic vessel, by ASLAK LIESTÖL (Oslo). *Ibid*, 163-70. [Important for Dark Age students].

THE TOMB OF HETEP-HERES, the mother of Cheops, by the late G. A. REISNER, completed and revised by W. STEVENSON SMITH. Harvard University Press (Oxford U.P.), £14. [Announced as forthcoming].

THE PALACE OF MINOS, KNOSSOS: a Handbook, by the late J. D. S. PENDLEBURY. Max Parrish, London, 12s 6d. [A new edition of a guide-book indispensable for all visiting Crete].

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<sup>4</sup> See Dr Röder's article in *Germania*, 31, 1953, pp. 24-7.



## The Britons in Southern Scotland\*

by KENNETH JACKSON

THE archaeological background of the people of what is now Scotland south of the Forth and Clyde in the Roman period was a La Tène one, and specifically chiefly Iron Age B. This links them intimately with the Britons of southern Britain in the conglomeration of Celtic tribes who called themselves Brittones and spoke what we call the Brittonic or Ancient British form of Celtic, from which are descended the three modern languages of Welsh, Cornish and Breton. To the north of the Forth was a different people, the Picts. They too were Celts or partly Celts; probably not Brittones however, but a different branch of the Celtic race, though more closely related to the Brittones than to the Goidels of Ireland and (in later times) of the west of Scotland. Not being Brittonic, the Picts may be ignored here. Our southern Scottish Brittones are nothing but the northern portion of a common Brittonic population, from the southern portion of which come the people of Wales and Cornwall. Some historians speak of the northern Brittones as Welsh, following good Anglo-Saxon precedent, but this is apt to lead to confusion. The best term for them, in the Dark Ages and early Medieval period, as long as they survived, is 'Cumbrians', and for their language, 'Cumbric'. They called themselves in Latin *Cumbri* and *Cumbrenses*, which is a Latinization of the native word *Cymry*, meaning 'fellow-countrymen', which both they and the Welsh used of themselves in common, and is still the Welsh name for the Welsh to the present day. The centre of their power was Strathclyde, the Clyde valley, with their capital at Dumbarton.

The purpose of this article is to give some account of the history of these people. One thinks of the Picts as a remote nation of whom next to nothing is known, and yet oddly enough a good deal more is known about them during the period from the 6th century, when their medieval history begins, to the ninth when they ceased to exist as a nation, than about the Cumbrians during the whole of the much longer period from the 5th century to the 11th when their historical independent existence is attested. The difficulty is that our sources are very meagre, and some are of questionable authenticity. Some of them are historical documents properly so called, like the Irish annals and the Anglo-Saxon Chronicle, and we know pretty well how far we can rely on these. There are also the *Annales Cambriae*, redacted in their present form in the 10th century, but probably based in part on some kind of written material of the 7th and 8th centuries emanating from Strathclyde.

Others of our sources are not historical but literary, in the form of poems and tales handed down to us almost exclusively in Welsh. No doubt they existed also, and primarily, in Cumbric; but as the Welsh and Cumbrians shared the common language and culture of the *Cymry*, the heroic literature of the northern regions, which seems to have been much fuller than that of the southern, doubtless circulated also in the south. Like all early Celtic literature it would be primarily oral. With the dying out of the Cumbric language all its oral literature of course disappeared too, and such manuscripts as may have existed, being now unintelligible, would easily become lost; whereas in

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\* This article represents a lecture delivered to the Society of Antiquaries of Scotland, 8th March, 1954.

Wales the language survived and with it some of the oral literature, which was then written down when it became customary to do so, perhaps first in the 8th or 9th century.

These literary sources are of two kinds. One class consists of certain panegyric poems addressed to the princes of the Cumbrians in the 6th and early 7th centuries. Celtic scholars mostly agree nowadays, that in spite of the long gap between that period and the oldest surviving Welsh manuscript versions of them, of the 13th and 14th centuries, the majority if not all of them may well be genuine and contemporary, though some undoubtedly have secondary interpolations, and that they may therefore be regarded as historical evidence if they are used with great caution. Unfortunately they tell little, as they are not narratives but poems of praise and elegy. The two most important groups are the poems attributed to Taliesin, about a dozen addressed to northern chiefs of the last part of the 6th century; and the long poem *Gododdin* by Aneirin, which tells of the prince of Edinburgh about the year 600, and how he sent a picked band of Brittonic warriors against the English settlers of Bernicia and Deira, and of the tragic fate that befell them.

The other body of literary material is not contemporary poetry, but consists of saga traditions in prose and verse handed down by professional Welsh story-tellers. It is difficult to know how far we can rely on these, but they can sometimes be checked by outside evidence, and it seems certain they contain some kernel of historical truth. It is clear that a considerable body of such semi-historical tales and poems once existed, of which only a few scattered fragments now remain. They tell of the princes and heroes of what the Chadwicks have accustomed us to think of as the northern British 'Heroic Age'.

There are two documents which are not readily classified as either purely historical or purely literary. One of these is the *Historia Brittonum* of Nennius, which in chapters 57 to 65 gives information of great value on northern Britain in the 6th and 7th centuries. Nennius himself was writing early in the 9th century, but he knew and used historical material probably put together in Strathclyde in the second half of the 8th century and itself based on written sources apparently of the 7th, perhaps going back to the early 7th and therefore right up to the period told of in the literary traditions. These Strathclyde sources were probably connected, though not identical, with those used by the *Annales Cambriae*. That they were written, not oral, is shown by the form of certain northern names and words like *Cunedag*, *Birdei*, *Atbret*, etc. *Birdei* is of course the proper Pictish form of the name of the Pictish King given elsewhere as *Brude*; and I should like to point out, what no one seems to have remarked, that Nennius seems to preserve the Pictish name of the place where he won his great victory over the English in 685, that is *Lin Garan*, 'The Pool of the Herons'. It is most improbable that the Welsh, or even the Cumbrians, would have known of or had any name for this remote and insignificant puddle until it was made famous by the battle, when the men of Strathclyde would then hear of and record it under its Pictish title. There is no reason philologically speaking why *Lin Garan* should not be Pictish. Details such as this, or the nickname *Flesaur*, 'The Artful Dodger', given to Æthelfrith, or the preservation of Brittonic names of famous northern battles known otherwise to us only by Anglo-Saxon titles, are features of the northern material in Nennius which tend to establish its early and authentic character. Besides this, however, it is evident that Nennius also drew upon the northern literary traditions, in much older and fuller versions than any now available to us, and giving details now otherwise unknown. The other document not readily classified is the genealogies of Cumbric chiefs of the 5th, 6th and 7th centuries found in Welsh manuscripts. Such genealogies were preserved and handed on orally by family historians, and



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there is some possibility that most of the northern ones we have were the work of early 7th century historians of this type, though it is on the whole more likely that they were put together in the 9th century out of the information available in the sagas of the Cumbrian Heroic Age. The pedigree of the royal family of Strathclyde, however, which is much longer than all the others, is almost certainly a genuine family tradition from the start, handed on by the royal bards ; it can be checked at various points from reliable historical sources.

Such then is the chief material which we can use in studying the history of the Brittones of the North. There is no necessity to discuss here the Roman period proper ; but it is clear that the country between the Antonine and Hadrian's Walls, which for a time formed an actual part of the Roman province, was subject to a considerable degree of Roman influence all through Roman times, and this must have been an important factor binding the Brittones of the Lowlands still more closely to their kinsmen south of Hadrian's Wall, and tending to distinguish them from the Picts beyond the Antonine Wall. The tribal and place-names preserved in Greek and Latin sources prove beyond question not only that the inhabitants were Celts, but that they were P-Celts not Q-Celts, that is to say, were Brittonic. The town of the Selgovae called by Ptolemy *Carbantorigon*, and *Pexa* on the Antonine Wall in the Ravennas list, both bear P-Celtic names. Two particularly interesting cases in Ravennas somewhere in our area, unfortunately not identifiable, are *Tadoriton* and *Maporiton*, 'The Ford of the Father' and 'The Ford of the Son', both of which again are definitely P-Celtic. Who the Father and Son were, and why they had fords, is a tantalising mystery. The Romano-Celtic name of the modern Birrens in Dumfriesshire, *Blatobulgium*, is evidently a nickname. It means 'The Sack of Flour'. Why it was so called one cannot tell—but is it any stranger than the name of the town of Medicine Hat in Canada ?

The curtain begins to roll up from the history of the Cumbrians in the 5th century. Welsh tradition in Nennius and the Genealogies preserves a story about the chief Cuneda who came from Manau Guotodin, probably round Stirling, Clackmannan, and West Lothian at the end of the Antonine Wall, and migrated to Wales, where he expelled the Irish and founded a number of dynasties there which lasted for several centuries. As we all know, this has been interpreted as meaning that he and his followers were moved as a piece of deliberate Roman policy and settled in Wales as Roman *foederati* to check the Irish squatters who had been marauding and taking land there. This is ingenious and plausible, though the theory on the Roman nature of the move has been rather shaken by Chadwick's demonstration<sup>1</sup> that the date usually given, about A.D. 380, must be wrong, and that it cannot really have taken place much before about 450. If this was a Roman act it implies that the Romans controlled Lothian, and means an alliance. Now scholars have noted that the three generations immediately preceding Cuneda in his pedigree bear Latin names, as well as two of his sons and one of his grandsons, and this has led some to think of a very considerable degree of Roman influence, reaching right back to the beginning of the 4th century ; though, as Chadwick points out, if the genealogy is reliable so far back they can hardly themselves have been Romans, since for six generations before that the names are all of a type which we know as characteristically Pictish—not surprising in a princely family living right on the very borders of Pictland. The hypothesis is held to be strengthened by the fact that Cuneda's grandfather is Patern Pesrut or Paternus of the Red Tunic, who would have flourished in the middle of the 4th century, and this is taken to mean that he held some imperial Roman office and had been

<sup>1</sup> H. M. Chadwick, *Early Scotland*, p. 148.

invested with some kind of Roman purple robes to encourage his loyalty, all as part of a policy of keeping the barbarian border states friendly. Still further, the title of *guletic* by which Cuneda is known has been thought by some to be the equivalent of some Roman rank like Comes Britanniae or Dux Britanniarum, though I hardly think that many nowadays would suppose that he actually held such an office.

This opinion is strengthened by the contemporary evidence for the other end of the Antonine Wall. The well-known letter of St. Patrick, of the middle of the 5th century, shows us a Coroticus king of Dumbarton and his followers who were regarded and regarded themselves, as Patrick clearly shows, as Roman citizens. This man's pedigree is also preserved—it is that of the historical kings of Strathclyde—and in it his father is Cinhil and his grandfather Cluim, which *may* be the Latin names Quintilius and Clemens. Besides, he too is called *guletic*.

Chiefly on all this evidence, a theory has been constructed that towards the end of the Roman period there was a considerable extension of Roman influence northwards, from Hadrian's Wall to the Forth, and that part of the northern frontier army and equipment was handed over to friendly and allied native princes, probably by or under Magnus Maximus, so that they could defend the now otherwise defenceless northern frontier—presumably with instructions to be good boys and not turn against the tempting Roman province themselves. This sort of interpretation of early Celtic historical sources, trying to squeeze every drop of a possible Roman nature out of them, has been fashionable ever since Collingwood's time. Speaking from the Celtic side, one would like to counsel caution. It all depends on the Old Welsh pedigrees and on a passage in Nennius which is itself of a genealogical tradition. But how far back is this Welsh genealogical tradition reliable? Can we really trust it in detail in the 5th century, still more the 4th? And how much do the Latin names mean? They certainly suggest Roman influence, but Latin names are not uncommon in the 5th and 6th centuries in most of the early Welsh pedigrees, as well as in the inscriptions of that period from Wales, Cornwall, and the North; and these people are surely not all to be taken as holding Roman office, but bear witness simply to the inevitable prestige of Rome among the barbarian tribes on its borders. The connection of the word *guletic* with any Roman title is extremely precarious; and others besides Roman officials may have liked wearing red tunics. The Latin character of some of the names is exaggerated. Cinhil and Cluim may not be Latin at all. Another 5th century ancestor of a Northern dynasty, Coel Hen or 'Coel the Old', has been made into a Roman, one Caelius, and as 'Caelius Votepacus' he seems to have passed permanently into the mythology of Dark Age historians. I should like to take this opportunity of protesting that the name Coel cannot come from Latin Caelius<sup>2</sup>; and, though I should by no means attempt to deny the theory of Romanised British princes set up as defenders of the province in the 4th and 5th centuries, it would be wise to adopt a sceptical attitude and a not too ready credulity.

For the earlier 6th century our sources are meagre indeed. The chief is a series of Latin inscriptions, four in Lothian and the Merse, three at Kirkmadrine in the Rinns of Galloway, and two at Whithorn. One of the Whithorn stones may be as late as the 7th century<sup>3</sup>, but all the rest belong probably to the first half of the 6th or in one or two cases perhaps as early as the late 5th. The Kirkmadrine and Whithorn inscriptions are clearly those of Christians, and are doubtless connected with the missionary church in Galloway associated with the name of Ninian. Whatever Ninian's dates may have been,

<sup>2</sup> cf. *my Language and History in Early Britain*, p. 335 f.

<sup>3</sup> The *Initium et Finis* stone.



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the inscriptions prove the presence here of Christians as early as the beginning of the 6th century, though I cannot agree with my friend Mr Raleigh Radford that the Latinus stone at Whithorn can possibly be so early as the middle of the 5th, on epigraphic grounds. For the eastern series, two of them, the Kirkliston and Yarrowkirk stones, were both apparently associated with what seem to have been Christian cemeteries, and another, Manor Water, bears a cross. These early 6th century inscriptions clearly show, therefore, the fact that the Britons of southern Scotland, or some of them, were now Christians, whether as the result of direct inheritance from Roman times or as an aspect of the mission of Ninian. In the Gododdin poem, about 600, the Britons of Edinburgh are quite definitely Christian, in contrast to their enemies the English who are spoken of as a horde of heathens. The Yarrowkirk stone has recently been satisfactorily deciphered, and the following version is now agreed, by Mr Raleigh Radford, Mr Angus Graham, and the writer: 'This is the everlasting memorial; in this place lie the most famous princes Nudus and Dumnogenus; in this tomb lie the two sons of Liberalis'.<sup>4</sup> I should like to emphasise that Rhys's idea that the inscription commemorates two sons of a northern chief known in the early Welsh genealogies as Nudd the Generous is impossible, not only because of the meaning of the inscription but also because Nudd Hael must have died in that case about a hundred years later than his sons.

The end of the 5th century and beginning of the 6th is the period during which King Arthur is supposed to have flourished. There is a school of thought which believes that Arthur was a chief of northern Britain, and that his famous battles were all fought in the north. I have argued elsewhere<sup>5</sup> that the philological evidence relied on by the proponents of this view does little to support it, and as I shall be amplifying this argument in a forthcoming article<sup>6</sup> no more need be said about it here. I should like to refer however to the Welsh tradition which can be traced back to the 11th century which shows that some sort of story or legend was current about a feud between Arthur and a chief of southern Scotland called Hueil, whose brother was Gildas the monk and whose father was the person known as Caw of Pictland. The legend of Caw is that he was a robber giant who lived in Pictland to the north of the mountain called *Bannawg*, 'Peaky'; that he was killed on a raid into Britain south of the mountain; that he was resurrected by St. Cadog, and fathered his son Gildas in Strathclyde. The story may be mentioned here for two reasons. First, it hints at the existence of tales about Strathclyde now almost entirely lost. Second, it helps us to locate the mountain of Bannawg, which is spoken of elsewhere in medieval Welsh literature, which knows nothing of the Antonine Wall, as the great dividing boundary in Scotland between Pictland and the rest of Britain. There seems little doubt that it is the range of heights in which the *Bannock* Burn rises, which almost entirely block the narrow isthmus between the Forth near Stirling and the Clyde near Dumbarton, to the north of the Antonine Wall, and which must have constituted an uninhabitable no man's land dividing the Picts and Gaelic Scots from the Britons.

With the second half of the 6th century we enter the British Heroic Age, as defined by the Chadwicks. Welsh poetry and tradition now begin to tell us, though with tantalising gaps, something about people and events which one can really begin to feel with

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<sup>4</sup> A full account will appear in the Inventory of the Ancient Monuments of Selkirkshire now in preparation by the Royal Commission on Ancient Monuments (Scotland).

<sup>5</sup> *Modern Philology*, XLIII (1945), 44 ff.

<sup>6</sup> 'The Arthur of History' in the forthcoming guide to Arthurian studies edited by R. S. Loomis.

some confidence contain a genuine kernel of historical truth. The best known character, because he was Taliesin's hero and patron and because details from some lost traditional saga of him survive in Nennius and later Welsh poetry, is Urien of Rheged. Rheged was one of the kingdoms of the Cumbrians. Its whereabouts are very uncertain. The name has been seen in Dunragit in the Rinns of Galloway and in Rochdale in Lancashire. The former seems to be generally accepted, and Rheged is commonly identified more or less with Galloway. But in the tradition of the 12th century Welsh poets it was remembered that Carlisle was in Rheged. The Taliesin poems speak of Urien as prince of Catraeth, which is probably Catterick near Scotch Corner; he fights the Bernicians on the shores of Northumberland. He is associated also with *Aeron*, which *may* be Ayrshire, and with two other places, *Yr Echwydd* and *Llwyfenydd*, though it is unknown where these were.<sup>7</sup> The possible range of Rheged is thus very wide; but it seems probable that its heartland was the Solway plains round Carlisle and Annan, and the Eden valley, and that Urien's lands may have stretched from the Eden over Stanemoor to the neighbourhood of Catterick. Whether they also included all or any of mid-Lancashire, Ayrshire, and the Rinns of Galloway, seems highly uncertain; at any rate they did *not* include Strathclyde.

The poems on Urien and his son Owain attributed to Taliesin, most of which may well be contemporary, do not tell us much about them. They name a number of their battles, of which some if not all were against the English, who in Urien's time were beginning to occupy Bernicia in some force. One of these may have been the 'battle at the huts of Brewyn', which, as I have shown elsewhere<sup>8</sup>, may be the Roman station of Bremenium in the Cheviots; but the 'battle in the ford of Dumbarton' may have been part of some civil conflict, unless it was against the Gaelic Scots. In one or two poems an English chief is mentioned, not by name, for that is very rare, but by nickname; *Fflamddwyn*, 'Flame-Bearer'. This has found its way into school history books as a title of Ida; strangely, for Ida, who died in 559, is certainly too early.

The saga of Urien, fragments of which are preserved in Nennius and later Welsh poetry, must have been an important historical document. From what survives, we learn from Nennius that Urien and his sons fought with Theodric son of Ida, king of Bernicia 572-579; and that Urien along with three other British kings, Riderch Hen, Guallauc, and Morcant, fought Hussa, Theodric's brother, king of Bernicia 585-592, and besieged him in Lindisfarne island; but that Urien was betrayed by Morcant through jealousy and was killed. The Welsh poems add some interesting and doubtless authentic details; in addition to Morcant, his enemies were Gwallawg son of Lleennawg (Nennius' Guallauc) and Dunawd son of Pabo, northern chiefs known elsewhere, and others; that his murderer was an unknown Llofan Llaw Ddifro; and that he was killed at Aber Lleu, 'the mouth of the Lleu'. This is plausibly identified with the little river Low which runs into the sea opposite Lindisfarne. Such details seem good evidence that the lost saga of Urien was based on historical fact.

Riderch Hen is well-known in Welsh tradition, as Rhydderch Hael, and moreover as in the case of his ancestor Ceretic we have independent confirmation of his existence, for he is the *Rodericus filius Tothail qui in Petra Cloithe regnavit* of the Life of St. Columba—'Rhydderch the son of Tudwal who ruled in Dumbarton'. Since he lived in Columba's

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<sup>7</sup> It is not impossible that the name *Llwyfenydd* may survive in the name of the river *Lyvennet* which runs into the Eden not far above Penrith. Cf. Ekwall, *English River Names*, p. 276. Ekwall's doubts about *Lemaniio-* are unnecessary, as *i-* affection would be quite possible here, see *Language and History in Early Britain*, p. 616.

<sup>8</sup> ANTIQUITY, XXIII, 48-9.



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time he must have been reigning before 597. He was king of Strathclyde and collateral ancestor of the later historical kings. He appears also a number of times in the 12th century Latin Lives of St. Kentigern; and in the Kentigern material, as well as in the Welsh legend of Myrddin, he has relations with the mad Strathclyde prophet who finally turns up in the medieval Arthurian romances as Merlin. In connection with Rhydderch one must mention his contemporary Aedhán son of Gabhrán, king of Scottish Dál Riada, the defeated leader of the Scots at Degsastán in 603. It is well-known that Aedhán had a son Arthur and a grandson Rígullón, Brittonic names which point to some British family connection, and in fact later Welsh tradition, for what it is worth, gives Aedhán a British mother<sup>9</sup>. But if the Welsh traditions can be trusted he must have fallen out with the Britons, for a poem<sup>10</sup> belonging to the Strathclyde legend of Myrddin refers to a raid made by Aedhán on Strathclyde in Rhydderch's time, and perhaps also to a battle in Galloway. This story evidently explains the fact that Aedhán was remembered in the medieval Welsh Triads as Aeddan Fradawg, 'Aedhán the Treacherous', who led a costly raid against Rhydderch at Dumbarton.

Another northern tale which must have existed in the form of a saga, traces of which are to be found in Welsh sources, is the story of the battle of Arfderydd. The *Annales Cambriae* simply give *Bellum Armterid* under the year 573, but later Welsh material tells that this battle was fought between Gwenddoleu son of Ceidiaw and his cousins Gwrgi and Peredur, and that Gwenddoleu was killed. A 12th century Latin text connected with St. Kentigern seems to show that the battle of Arfderydd took place in the meadows on the English side of the Liddel near the Roman Castra Exploratorium, by *Arthuret* near Longtown. The Welsh Triads tell how Gwrgi and Peredur were abandoned by their troops and killed when fighting an unknown chief called Eda of the Big Knees at an unidentified place called Caer Greu; but the *Annales Cambriae* give only the date of their death, in 580. Nothing is known about where they belonged, though if the place-name Carwinley near Castra Exploratorium is really Caer Wenddoleu<sup>11</sup>, Gwenddoleu would have come from the Arthuret district.

A number of other chiefs of the northern British Heroic Age are known. The most interesting are Clydno of Eidyn, his son Cynon, and his brother Cadrawd of Calchfynydd. Eidyn is Edinburgh, and Clydno was presumably some noble of the neighbourhood; Cynon is the most prominent hero in the Gododdin poem, and was apparently one of the very few of the men of Edinburgh, if not the only one, who escaped alive from the disaster at Catterick. Calchfynydd has been identified with Kelso<sup>12</sup>, and if so, this would suggest that the family of Clydno and Cadrawd, not to mention their brothers Cynfelyn Drwsgl and Cynan Genhir, were spread all through Lothian shortly before the whole district was occupied by the Angles. But the identification with Kelso is shaky<sup>13</sup>, and ought not to be stressed. Cynfelyn Drwsgl occurs in the Arfderydd story on the side of Gwrgi and Peredur, his first cousins once removed in the genealogies, which might possibly hint that they too came from somewhere in Lothian.

<sup>9</sup> Cf. Wade-Evans, *Vitae Sanctorum Britanniae*, p. 315.

<sup>10</sup> Edited by A. O. H. Jarman, *Bulletin of the Board of Celtic Studies*, xiv, 106 ff.

<sup>11</sup> See *English Place-Name Society*, xx, 52-3.

<sup>12</sup> See Skene, *The Four Ancient Books of Wales*, i, 172; Watson, *The Celtic Place-Names of Scotland*, p. 343.

<sup>13</sup> *Calchfynydd* means 'lime-mountain', and *Kelso* is apparently named from the chalky cliff by the town, called *Chalkheugh* in the 18th century. With carboniferous limestone a well-known feature in the Lowlands, the logic of this proof is not apparent. Besides, the cliff is hardly a *mynydd*.

The last piece of information which we have in Welsh referring to the northern Heroic Age is a fragmentary interpolation into the Gododdin poem. A couple of verses, the one a mere variant of the other, mention the killing in battle of one Dyfnwal Frych. It has long been recognised that this must be a reference to the battle of Strathcarron in 642, at which the Strathclyde Britons under Owein son of Beli son of Neithon killed Domhnall Brecc the king of the Scots of Dál Riada, and brought the aggressive activities of the Dalriadic kings to a temporary close. Ifor Williams has recently suggested<sup>14</sup> that an obscure line in these verses may contain the phrase 'they were wroth with the grandson of Nwython', that is, with Owein. This wandering fragment, which somehow found its way into the story of the battle of Catraeth about 40 years earlier, bears unequivocal testimony to the existence of heroic poetry in Strathclyde in the middle of the 7th century.

About this time and shortly before, the Angles of Bernicia, who had been settling the country between the Pennines and Cheviots and the sea in Northumberland already for about a century, began to push their way into Lothian and up towards the Forth, so that Gododdin, the British country of the old Votadini, became now for the first time English; which it was to remain for the next four centuries. The English capture of the key fortress of Edinburgh is probably indicated by the entry in the Irish annals referring to a siege of *Etin* in 638, during the reign of Oswald, king of Northumbria. The predominantly English character of the place-names of south-eastern Scotland, including a high proportion of obviously very early ones, goes back of course to this time and the ensuing centuries; nevertheless some Brittonic may have continued in use in the less accessible hilly districts. Certainly the Angles borrowed many Brittonic place-names from the natives, as may be seen by a glance at the modern map with its names like Peebles and Melrose and Pencaitland and Tranent, Linlithgow and Pennygant. Not very long afterwards, probably in the reign of Oswald's successor Oswy, 641-670, the Northumbrians took possession of wide areas of the Pennines and Cheviots; the whole of northern Lancashire, Westmorland, Cumberland, Dumfriesshire, and Galloway, and quite likely also Carrick. Nevertheless, the comparative rarity of English place-names in South-west Scotland, contrasted with their much greater frequency in the south-east, does suggest that the English occupation of these widespread western districts was more in the nature of a scattered upper crust of landlords rather than a really thick settlement of peasants. It may well be the case, as Wade Evans has proposed<sup>15</sup>, that Oswy acquired Rheged by the marriage with Rheinfelt, great-granddaughter of Urien, which is mentioned by Nennius, and not necessarily entirely by conquest and occupation. One would assume therefore that the Cumbrian language did not entirely die out in these western lands taken by the English, and there is some reason to think that this is so, at any rate in some of the hilly regions of Cumberland, where Cumbrian place-names are relatively frequent. They are found of course in many parts of this whole area, as we see in cases like Carlisle, Penrith, Derwent, Ecclefechan, Lochmaben, Penpont, Ochiltree, Trostrie, and so on. As a result of all this the wide lands of the independent Britons of the whole of southern Scotland and North-west England were reduced in a single generation to little more than the valley of the Clyde and the surrounding uplands, where however they held out manfully for centuries.

<sup>14</sup> *Transactions of the Cumberland and Westmorland Antiquarian and Archaeological Society*, new series, LI (1950), 80 ff.

<sup>15</sup> *Transactions of the Dumfriesshire and Galloway Natural History Society*, XXVII, 82.



## THE BRITONS IN SOUTHERN SCOTLAND

For the remaining history of Brittonic Scotland we lose the guidance of Welsh tradition, and have to depend on the conventional historical sources like the Irish, English, and Welsh annals. Between the wide expansions of the reigns of Oswald and Oswy, and the middle of the 8th century, there are references to battles between the men of Dál Riada and the Britons of Strathclyde in 678, 711, and 717, and between the Picts and the Britons in 744 and 750; but the places where and the reasons why they were fought, as well as their ultimate results, are alike unknown; except that the victory of Mygedawg in 750, which may have been at Mugdock near Dumbarton, seems to have contributed to a temporary slackening in the aggressive policy of the Picts under Onuist son of Wurguist (Oenghus son of Ferghus), the chief brunt of which had been borne by the men of Dál Riada.

In the same year Eadbert king of Northumbria annexed Kyle, and in 756 he joined forces with Onuist the Pict and crushed the men of Strathclyde. These events are the high watermark of English power in southern Scotland, and it must have looked for the moment as if they were going to absorb permanently all Scotland south of the Forth and Clyde. But two things saved the day. One was a crushing defeat inflicted on Eadbert by the Britons as he was retiring from Dumbarton, and the other, more important, was the period of dynastic struggle which set in in Northumbria on his death in 758. For the next hundred years we know almost nothing at all about the history of Strathclyde. Then comes the long anarchy and disruption brought about in northern Britain by the Norse and Danish raids and settlements. The Danish kingdom of York was established in 867, resulting in the isolation of Bernicia from the rest of England and the collapse of Northumbria as a great power. In 870 the sons of Ragnar Lothbrok plundered and destroyed Dumbarton, which must have been a heavy setback for Strathclyde; but on the whole the Scandinavian troubles worked to the advantage of Strathclyde, in seriously weakening Northumbria and keeping busy the men of the new kingdom of Scotia, the combined Dalriadic Scots and Picts. Indeed in 878 the royal family of Dumbarton seems to have gained a temporary control over Scotia, in the person of Eochaidh, son of Rhun king of Strathclyde by the daughter of Kenneth MacAlpine. He and his mysterious regent Girig seem to have held the kingdom of Scotia for eleven years, presumably with the support of some party of the Scottish chiefs; but in 889 they were ejected. Eochaidh is the last member of the royal family descended from Coroticus whose exact descent we know, preserved in the fifth of the Old Welsh 'Harleian' pedigrees. This genealogy is certainly reliable at least as far back as Beli about 600, since it is corroborated at several points by the independent evidence of the Irish, English and Welsh annals. The fact that it does not continue in the Harleian manuscript beyond Rhun, who became king in 872, does not mean that the dynasty then came to an end, though we do not know precisely how it ties up with the royal family as we know it in the 10th century. The reason is simply that the Harleian genealogies were first compiled, it seems quite clear, during Rhun's own lifetime, on behalf of his Welsh contemporary Rhodri Mawr.

Towards the end of the 9th and early in the 10th century Northumbria was still further weakened by two important Scandinavian colonies in the west. Probably the earlier was the occupation of Galloway and Carrick by the Gall-Ghóidhil, men of mixed Norse and Gaelic stock, apparently coming in the main from Argyll and Bute. The settlement they established was evidently independent of Northumbria and Strathclyde alike, and continued in semi-independence of the kings of Scotland as late as the middle of the 13th century, somewhat like the very similar mixed Norse-Gaelic kingdoms in Man and the Hebrides during the same period. In spite of their Norse origins the men of Galloway and Carrick must have been predominantly Gaelic speakers, and they

continued to speak Gaelic until the 17th century. If the Cumbric language had outlasted the Northumbrian period in any part of this area it is likely that it was swamped by Gaelic now, and Galloway, which must have given up much of its Brittonic character in the 7th century, must have lost altogether what remained in the late 9th; a fact which is reflected in the comparative rarity of Brittonic place-names there. The second important Norse colony, again heavily mixed with Gaels but in this case with Gaels from Ireland, appears in the coastlands of Lancashire and Cumberland and in the Lake Hills in the early 10th century, and provided a strategic link between the Danish kingdom of York and its starting off place in Dublin. As with Galloway, the Brittonic of Cumberland, which may well have survived the Northumbrian occupation in the remoter areas, is likely to have been largely killed off by this considerable influx of Scandinavians.

As a result of this Norse activity in the west, Northumbria west of the Pennines and Cheviots seems now to have been reduced to Dumfriesshire and the inland parts of Cumberland and Westmorland, chiefly the Eden valley. But the whole of this area must have been very weakly held by Bernicia, having become strategically untenable in the circumstances of the time. Consequently the Britons of Strathclyde were able to profit from this and to extend their power southwards throughout this region, edging down the Annan and up the Eden between English Bernicia on the east and the Norse colonies in Galloway and Cumberland on the west, as far as the Derwent and Eamont rivers in northern Cumberland and up to the Pennine ridge at Stanemoor, where the 'Rerercross' now became a traditional boundary. This remained the extent of Cumbria for close on two centuries. The consequent re-Brittonization of Dumfriesshire and northern and eastern Cumberland is likely to have brought a fresh influx of Cumbric speakers from Strathclyde, and to have given a new lease of life to such Cumbric as may still have survived there; and the evidence of some place-names, which seem to point to a Cumbric stratum overlying an English one, supports this view.

The English come-back against the Danes in the 9th century, under Edward the Elder, Athelstan, and Edmund, had its repercussions in Strathclyde. In 921 or 920 the men of Strathclyde, under what king we do not know, joined the Scots, Bernicians, and Danes of York in submitting to Edward the Elder as a consequence of his northern campaign, though he did not think it strategically possible or advisable to attempt to annex the newly extended kingdom of Strathclyde, outflanked as it was by the Scandinavians of York, Cumberland and Galloway. In 927 his successor Athelstan again got the submission of the Scots, Bernicians, and Strathclyde Britons, at a meeting at Dacre on the new Strathclyde border near Penrith. The king at this time was a certain Owein, and with him we are able to take up again the pedigree of the royal family of Dumbarton after the gap in our knowledge following the death of Rhun. In 934 or 933 Owein was defeated by Athelstan during his campaign in Scotland; and again in 937 Athelstan routed the men of Strathclyde along with the Scots and Northmen at the great victory of Brunanburh.

We do not know who was the British king at Brunanburh, though it is often stated, without any evidence, that it was Owein; but in 940 or 941 a certain Donald, that is Dyfnal, appears as king. The kings Donald or Dyfnal in Strathclyde in the 10th century are rather bewildering. There was one who died and another who succeeded during the time of Constantine II of Scotia, 900-943, and the second of them may well be the Donald of 940 or 941. Nothing is known of the first one's parentage or how he joins on to the Strathclyde pedigree, but the second may be the same as the Donald son of Owein (doubtless the Owein just mentioned) who was still king later in the century. If so, the statement in the Pictish Chronicle that he was son of Aedh would be wrong; and in any case the idea that he was Constantine's brother, and therefore a Scot, seems to be without



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any real foundation, and the statement sometimes made that he came to the throne in 906 or 908 is entirely so.

Soon after, the kings of England again asserted themselves in the north, when in 945 Edmund harried Strathclyde, blinded Dyfnal's two sons, and handed the kingdom over on lease to Malcolm I King of Scots. The reason for this attack was apparently that the Strathclyde men had been continuing the policy of aiding the Northmen as at Brunanburh; and our sources make it clear that the purpose of the cession to Malcolm, who must have abandoned that policy so far as Scotia was concerned, was to have him act as a buffer between the Northmen of Dublin and those of the Danelaw, and protect the English rear in an area where it was in any case strategically difficult for Edmund to make his power felt. In this way the Gaelic kings of Scotia for the first time gained a foothold south of the Forth-Clyde line.

The new arrangement seems not to have outlasted the death of Malcolm I in 954, for in 971 the Britons under Rhydderch son of Dyfnal invaded Lothian and killed the Scottish king Cuilén, and then defeated an attempt at retaliation by Cuilén's successor Kenneth II. Whatever may have been the nature of their subjection to Malcolm I, they had evidently regained their independence by this time. Two years later we find a certain 'Malcolm, King of the Cumbrians', apparently another son of Dyfnal, doing homage to King Edgar at Chester. In 975 Dynwallawn son of Owein, king of Strathclyde, went on a pilgrimage to Rome and died; he seems to be the same person as the Dyfnal father of Rhydderch and Malcolm, who in 971 and 973 must have been acting as regents for their father in his old age; that is, if he is the same as the Donald of 940-41 and the Dyfnal of 945, which would make him an old man by now. In any case Strathclyde was still independent under its own kings. In 997 Malcolm son of Dyfnal died as king of the North Britons, and was evidently succeeded by his brother Owein the Bald. In 1000, Ethelred the Unready raided and harried all Cumbria, probably because it had been playing the old game of collaborating with the Northmen against England. Then in 1015 or 1018 (the date is uncertain) Owein joined with Malcolm II of Scotia in the campaign culminating in the battle of Carham, which led to the confirmation of the cession of Lothian to Scotland probably first granted by the English in 975. Owein died about this time (he may have been killed at Carham), and with him the long line of independent kings of Strathclyde, probably going back in unbroken succession to Coroticus in the 5th century and beyond, evidently came to an end. The next king of Strathclyde was apparently Duncan, grandson of Malcolm II of Scotia, who must therefore have been installed in Cumbria by his grandfather, who would have got control of the kingdom, on the death of Owein, in some way unknown to us. It seems clear that from now on Cumbria became a mere province of Scotland. Thus in one rush the boundaries of Scotia were advanced from the Forth and Clyde to the Tweed and Derwent; the kingdom of Scotland began to take on its final shape; and the ancient kingdom of Cumbria ceased to exist.

One immediate result of this sudden expansion of the Gaelic element in Scotland must have been that with the consequent granting of lands in Lothian and Cumbria to Scottish lords a considerable superstratum of Gaelic speakers must have begun to establish itself in the Lowlands. This is shown quite clearly by the numerous Gaelic place-names found all through the Lowlands (not counting Galloway and Carrick, which had become thoroughly Gaelic over a century before), and even to some extent in the Brittonic parts of Cumberland, where few of them are likely to have been brought by the earlier Norse-Irish colony.

This brings us to one final question. How long did Cumbric continue to be spoken in the old kingdom of Cumbria, exclusive of Galloway and Carrick? Here it is necessary

to distinguish Strathclyde proper, which had never been Anglicized, from Dumfriesshire and northern Cumberland which had been under the Bernician Angles from the mid-7th to the early 10th century, and were only then re-Brittonicized from the north. It is likely that English must all along have been at least a prominent language in this southern region; and when in 1092 William II advanced the English border to approximately its present position on the Solway, re-built Carlisle, and planted the country with English settlers, the fate of such Cumbric as might still have been spoken there was sealed. Nevertheless there is one respect in which it is not dead even yet, for it is well known that the old Cumbric numerals have survived very extraordinarily to modern times among the Pennine shepherds of Cumberland and the West Riding, for the purpose of counting sheep, and that they are nothing but a garbled version of something which must have been identical with the numerals in Welsh. As for Strathclyde proper, it must presumably have been solidly Cumbric in speech until it fell into Scottish hands after Carham, but to judge from the place-names it must have become rather fully and rapidly Gaelicized after that, for in Lanarkshire and Renfrewshire there are about twice as many Gaelic place-names as Brittonic. Possibly Gaelic influences had been penetrating even before the 11th century. This, and the Norman and English element in the population brought in as a result of the feudalization of Scotland in the 12th century, probably soon caused the final disappearance of Cumbric here, and hence of any type of Brittonic speech north of Anglesey. The last, and apart from place-names the only, remnant of the Cumbric of Strathclyde is preserved in the so-called *Leges inter Brettos et Scottos*, which used to be attributed wrongly to David I<sup>16</sup>. They are evidently older than his time, though perhaps not very much older; the most probable date for their formulation is the early part of the 11th century, when the kings of Scotia, having now gained control of Strathclyde, would want to regulate relations between their old Scottish and their new Cumbrian subjects. The *Leges*, which are in Latin, must have made use on the Cumbrian side of some older system of laws redacted in Cumbric, just like the Welsh laws of Hywel Dda redacted in Welsh in the 10th century; and they preserve three technical legal terms in Cumbric, all of which have close relations in the legal terminology of early Wales. They are *galnes*, 'blood-fine', the Welsh *galanas*; *mercheta*, a tax paid by a father to his lord on the marriage of his daughter (Welsh *merch*); and *kelchyn*, apparently some sort of fine paid at the same time as a blood-fine but perhaps originally a contribution paid when the king went on royal progress through his lands (Welsh *cylch*).

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<sup>16</sup> So Loth, *Revue Celtique*, XLVII, 389; Watson, *op. cit.*, p. 132; Förster, *Die Flussname Themse*, p. 82. Following these authorities, I made the same mistake myself in *Language and History in Early Britain*, p. 9; and I wish to take the opportunity of correcting this error, and of thanking my colleague, Professor W. Croft Dickinson, for pointing it out to me.



# South American Prehistory: a review\*

by L. PERICOT

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THESE two articles<sup>1</sup> which deal respectively with the chronological basis of Patagonian prehistory and with its rock pictures, are by the well-known prehistorian who today is Professor of Prehistory in the Faculty of Philosophy and Literature in the University of Buenos Aires. They set out a scheme of outstanding importance for the prehistory of Patagonia which strengthens our opinion, already backed by evidence from all over America, of the great antiquity of Man in the New World.

It is inevitable that, when a European archaeologist has transferred his activities to the New World, he should see everything with European eyes, and look for parallels with the cultures he knew in the Old World. Thus it is natural that, in the few years which Professor Menghin has spent in Argentina, his untiring energy should have produced finds and theories of undoubted interest. These are summarized in a table in the first article, which boldly attempts to correlate the cultures of Patagonia with those of the Upper Palaeolithic and later periods in Europe.

Patagonia and the southern regions of America are of exceptional importance in human culture, because they are a veritable *cul-de-sac* which could by no possibility have been populated from across the ocean or, according to the hypothesis of Mendes Correa, by way of the Antarctic. Its settlement must therefore have taken place long after the arrival of Man in North America, and its archaeological remains must therefore preserve traces of the first and later successive waves of immigration.

Argentine prehistory has suffered both from the exaggerations of some famous and popular investigators and also from the undue scepticism of their opponents. But the work of men like Bird in Tierra del Fuego and Auer in the Pampas, to say nothing of reported discoveries in North and Central America, has already begun to resolve the situation; nor must we overlook the determined stand of Argentinians such as Frenguelli, Castellanos, Vignati, Imbelloni and others.

Menghin's summer field-work in 1951 and 1952, especially that done in the region of Comodoro Rivadavia and Rio Deseado, has provided the evidence required, enabling him to attempt an arrangement of the Patagonian industries in their correct sequence. He thinks he has found enough industries to justify him in detecting a Lower Palaeolithic aspect on such sites as Goicochea, Alma Gaucha and La Generosa, all of which yielded flakes with or without retouching, but no traces of hand-axes. He also maintains that the stratum in the Eberhart cave (Ultima Esperanza) which gave rise to so much discussion must be included here as a Lower Palaeolithic survival.

More convincing are the results obtained from a study of the marine terraces in the Feruglio region which establish a series of six terraces beginning in the Pliocene period. Auer shows that there are four lower terraces; the first, at 18 metres, corresponds to the

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\* Translated from the Spanish by Beatrice Blance and the Editor.

<sup>1</sup> 'Fundamentos Cronologicos de la Prehistoria de Patagonia', *Runa*, Vol. 5 (1952), pp. 23-43; and 'Las Pinturas Rupestres de la Patagonia', *ibid.*, pp. 5-22; by O. F. A. Menghin. Published at Buenos Aires.

DATE	AGE	SHELL MOUNDS IN COMODORO RIVADAVIA	MILITARY ZONE & N. SANTA CRUZ	MAGELLAN ZONE (BIRD)	S. CHILE	EUROPEAN CULTURES	
+ 1000	H O L O C E N E	<i>Mytilus</i> shell-mounds on 3m. terraces and higher	EQUESTRIAN	TEHUELCHENSE	<i>Pit-House cult. of the Yamana (Bird)</i>	HISTORY	
0			Tehuelchense 3	= Bird V			
- 1000			Tehuelchense 2				
- 2000		Low Venus shell-mounds on 6m. terraces and higher	Tehuelchense 1	= Bird IV	Shell knife culture of the Alakaluf (Bird)	IRON AGE	
- 3000							BRONZE AGE
- 4000			Proto- Tehuelchense	= Bird III	↑	NEOLITHIC	
- 5000							
- 6000			↑ Casape- drense	Toldense II = Bird I		↑	
- 7000							
- 8000		Absence of shell-mounds	Toldense I Solanense			FINAL MAGDALENIAN	
- 9000	FINI- GLACIAL						
- 10000	Würm IV	High Venus shell-mounds on 40-50 m. terraces	Olivienne ↓ ?		Eberhardt Cave	MAGDALENIAN	
	INTER- STADIAL						
	GOTI GLACIAL						
- 14000?	Würm III	↓ ?					
	DANI GLACIAL					SOLUTREAN	

FIG. 1. CHRONOLOGY OF STONE AGE IN PATAGONIA, ACCORDING TO MENGHIN (1952) (ABRIDGED)



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Mankato<sup>2</sup> period of North America; the second, at 10 metres, marks the first post-glacial marine transgression (about 5000 B.C.); and those at 6 metres are later transgressions. Meyer and Menghin have studied the shell-mounds of Bahia Solano in relation to these terraces. Those found on the three-metre terrace contained chiefly shells of the genus *Mytilus*, and Menghin calls the corresponding culture Tehuelchensian. This culture has varying local facies, that in the interior differing from the facies on the coast, and theoretically three stages can be discerned: (1) pre-pottery, (2) primitive pottery, and (3) developed pottery. In the last there are burial-cairns containing objects of metal.

The primitive coastal Tehuelchensian culture in the Comodoro region is characterized by delicate broad-tanged flint arrowheads, tanged spearheads, sidescrapers, knives, and abundant small scrapers and blades. Other common artifacts are grinding stones, hammers, *bolas*, pointed stones (for detaching molluscs) and bone points. Uncommon are pottery, engraved stones, ceremonial axes and evidence of skull deformation, whose centre of distribution is in the north. The cylindrical axe is influenced by the farming culture of the Araucanians. The author does not discuss the problem of the origin of pottery. On the strength of finds from below a volcanic deposit in Lake Mascardi he accepts a date in the 3rd millennium B.C. for the beginning of the Tehuelchensian culture.

The industries found on the upper terraces give Menghin the opportunity of naming the pre-Tehuelchensian cultures. In the shell-mounds of the 6-18-metre terraces the genus *Venus* predominates. The associated industry consists of scrapers, sidescrapers, and pebbles with a cutting edge, but there is no pottery and no well-made tanged points. Menghin calls this industry Proto-tehuelchensian and he thinks it is the maritime *facies* of a continental culture, and that it may exist unrecognized elsewhere. To the same stage he assigns Culture 3 which Bird identified in the caves of the Magellan region. He attributes the 18-metre terrace to the last phase of the last glaciation.

There were no shell-mounds on the 25-30-metre terrace, but there were traces of occupation consisting of points with rounded bases, points, flakes with bifacial retouching, but no grinding stones. Menghin calls this industry Solanensian.

He thinks he has found yet older remains on a 50-metre terrace near Caleta Olivia. There are found shell-mounds with *Venus*, implements made of chalcedony-flakes, scrapers, some amigdaloids and some thick implements with a curved denticulated edge. He

calls this industry Oliviansian.

It would thus seem that the coastal terraces of Patagonia provide evidence for a succession of cultures from the last stage of the Ice Age down to historical times. But this is a hypothesis and it must be tested by finds from the interior, which should be possible in stratified caves. Menghin states that this is difficult because most of those he visited were without productive levels. Such have, however, been found in the caves of Los Toldos, about 200 kilometres south-west of Comodoro Rivadavia, and in those south of the river Deseado. Apart from the rock-paintings to be mentioned later, two of the caves have produced good stratified deposits.



FIG. 2.

BOLAS OF THE SOUTH  
AMERICAN TEHUELCHESIAN  
From *A History of Technology*,  
O.U.P., vol. 1, fig. 97, by kind  
permission

<sup>2</sup> The central date for the Mankato phase obtained from C 14 is about 9000 B.C.

In Cave 2 at Los Toldos was found a layer of volcanic ash, below which were two archaeological layers separated by a barren layer of river gravel 40 cm. thick. The volcanic deposit should correspond to the first cycle of eruptions which Auer puts at about 6000 B.C., and the gravel to a wetter phase which Menghin regards as to be equated with the finiglacial period. Thus we have a lower level corresponding to the Mankato period of North America. In the lower layer were found remains of horse, many bifacial tanged (but unbarbed) flint points, scrapers, etc. A *bola* stone (the oldest known *bola* in America) painted red suggests, with other evidence, that the cave-paintings in this region belong to this period. Menghin calls this assemblage Toldensian, and he calls a similar but poorer culture, found by Bird in the caves of the Straits region, Toldensian II.

The industry of the upper layer of Cave 2 is rougher than the Toldensian, but can be explained by the finds from Cave 3. Here Menghin found three fertile layers, the uppermost being separated from the middle one by volcanic ash corresponding to that in Cave 2. The middle layer is separated from the lowermost by a sterile layer. The lowermost layer contains an industry like that of the bottom layer of Cave 2, and a sub-triangular slightly asymmetrical point with bifacial retouch like those of the 25-30-metre terraces of Caleta Olivia. We are thus in Toldense I.

The industry of the uppermost layers of these two caves<sup>3</sup> has been called Casapedrenian. Its chief types are blades (sometimes with retouched edges), thick scrapers, notched blades—an assemblage recalling the European Aurignacian and Magdalenian industries whose people also painted on the walls of caves.

Menghin interprets the facts above narrated as follows. In Patagonia are remains of protolithic character corresponding to the primitive hunters and gatherers (represented today by the Fuegians) of the Late Palaeolithic and Epipalaeolithic periods. Then the advanced hunters arrived (represented by the Pampas People) and originated various cultures with a Late Palaeolithic appearance. These survived as Oliviensian, Solanensian, Toldensian and Casapedrenian. The Toldensian is technically the most advanced; from it was derived the Tehuelchensian—how is not yet known, but possibly by the Proto-tehuelchensian of Kolhue Kaike and the *Venus* shell-mounds. This Tehuelchensian culture lasted till 2500 B.C. and was influenced by the Araucanians and other peoples who were already food-producers. It is to be regarded as paraneolithic on a hunting basis but using pottery. It is not known when the Araucanians occupied the Neuquén and began to influence Patagonia.

The resemblance of the earlier cultures to the Upper Palaeolithic of Europe is a striking fact; even the painted hands (to be discussed later) occur. The bold but logical explanation offered is that both groups originated in a common Asiatic centre, from which emigrated in one direction the proto-Europeans and in another the proto-Australians (the Murrayans of Birdsell). The Pampas People, related to both European and Australian races, exemplify this migration. They were advanced hunters in Patagonia, coming from the Chaco and Uruguay, in the latter of which countries their remains have been found. The Altoparanensians of Southern Brazil, using rough stone hand-axes and mace-heads, are regarded by Menghin as food-producers who absorbed the hunters they found established there.

On the other hand Menghin accepts the conclusions of Auer on the chronology of the Magellan Straits region. Advanced hunters had arrived there about 7000 B.C.; but it was not until about 2500 B.C. that the islands south of the Straits were occupied by Canoeros Indians. That means that these cultures must have existed side by side in

<sup>3</sup> That is, the upper layer of Cave 2 and the two upper layers of Cave 3.



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Southern Patagonia for several millennia. The bone harpoons found by Vignati at Cabo Blanco point to the same conclusion.

Menghin's theories are summarized conveniently in a comprehensive chronological table. It is the first time that comparisons have been made on a large scale between the prehistory of South America and that of Europe by one who can speak with authority on both areas.

The second article follows the same line of thought but embodies a more concrete study. Menghin first outlines the history of the discovery of the Patagonian rock-paintings, ranging from Moreno's at Lake Argentino in 1877 to those on the Pinturas river found by Vignati and Rex Gonzalez in 1949. There appear to be none in the Magellan Straits region. To these must be added those found by Menghin himself during his field-work of 1951-2. In Los Toldos are two caves with paintings and in El Chiche three. Between the river Deseado on the north and the Gran Central Meseta are many caves with paintings still unexplored. There should also be painted caves in the valley of the Gallegos river. In 1952 Menghin described a cave with painted hands in Northern Patagonia, west of Lake Musters; and he states that he knows of others like it in the provinces of San Luis and La Rioja.

The commonest are figures of hands in the negative (PLATE V): they are of normal size, usually left hands, but are sometimes small and must then be children's; sometimes they show mutilated fingers. Figures of feet and of human beings and animals rarely occur, but in the Rio de las Pinturas region there are hunting and dancing scenes. The guanaco is the animal most often figured, but puma and ostrich also occur. It is uncertain whether horses or other extinct animals are figured. The following signs occur:—Circles and ovals, open or filled with dots; scutiform and serpentiform signs; ladders, Greek fret, crosses and other geometric figures; the footprints of ostriches (perhaps arrows). The usual colour is red, but some are black and a few yellow or white; green is rare, but occurs.

The date of these paintings is a difficult problem, solved by Menghin in accordance with his own views. There is occasionally a superposition of paintings, and it is assumed that the latest must be ancient because the Tehuelchians have forgotten that their ancestors were painters. Menghin considers that the oldest paintings date from the Late Palaeolithic Period; on Toldensian and Casapedrenian sites are fragments of colouring matter and grinding stones. The caves with painted hands—Cañadon de las Cuevas, La Martita—contain Late Palaeolithic and Epipalaeolithic strata. The red hands are the oldest, and the order of succession of the others is black, yellow, and white, the last being carelessly applied at a late date. The oldest signs seem to be somewhat later than the hands, and the 'footprints' and the dancing and hunting scenes are later still. Other scenes, such as that at La Gruta showing arrows being shot at the sun, must be relatively modern.

Rock-carvings in the open are common and the oldest date from the last phase of the paintings, continuing to the end of the Tehuelchian culture, which already showed Araucanian influence, in the 17th and 16th centuries B.C.

The negative hands went on being painted for a long period, down to the time of the old Patagonian pre-pottery neolithic. More recent are the groups with painted scenes and engravings related to the engravings known in N. Argentina. These last ones correspond to the time of the influence from the North, when pottery reached that region; this time falls within the 1500 years which preceded the conquest.

Menghin compares the hands and even other motifs with those occurring in European Palaeolithic art. There is a surprising resemblance between some of the Patagonian scenes (e.g. Rio Pinturas) and those of the South African Bushmen. According to Menghin the East Spanish (which he calls 'Hispano-African') and the Bushman art have a common origin in the Sahara. He regards the hands as part of a ritual of initiation, quoting much evidence in support from India and Oceania, the negative red hands found by Roeder in New Guinea, those in Ceram (Moluccas) and those in Arizona (which are white and negative, and of Basket-maker age<sup>4</sup>).

Here, therefore, we have in South America a cultural trait which resembles another occurring in the culture of the advanced hunting peoples, whose origin must be looked for in Central Asia. Here too, therefore, may lie the explanation of the palaeo-European features in prehistoric South America, and of the possible relationship between the Australoids of South America and the European race of Cro-magnon.

We think the readers of ANTIQUITY, particularly those in the New World, will have been interested in this summary of the facts set forth by Menghin. We should add that we were recently shown some of the material in Buenos Aires by Professor Menghin himself and were impressed by its palaeolithic aspect. But we have seen so many unsuspected survivals of palaeolithic techniques in Europe that we should not dare, on that ground alone, to accept so remote a date for the finds. But the multiplication of dated finds, especially in North America and Mexico, now decisively favours a relatively high dating, and thus seems to be decisive in the old controversy about the antiquity of Man in America, which must reach back into the 3rd phase of the Ice Age (Wurm III). Menghin's theory comes within the ambit of orthodox opinion, and if accepted strengthens the case for a higher chronology in North America which was obviously settled earlier than South America.

So, although they may seem too bold, we accept Menghin's correlations as useful, provided it is not forgotten that they are merely part of a working hypothesis, based on facts which are still not numerous; and that modifications will doubtless be needed.

In Mexico and the U.S.A. there is a similar hiatus between the Late Palaeolithic and Epipalaeolithic industries on the one hand and the well-known neolithic cultures preceding the conquest. There seem to be three possible explanations: (1) these earlier cultures are not so old as is imagined (2) they evolved slowly and survived for a long time (3) other, intermediate, cultures existed and have yet to be found.

The geological evidence is basic, because all the presumptions of palaeolithic age are based upon the date assigned to the volcanic ash layer or to the river gravels which in some caves lie above the strata containing artifacts. To this extent we are now better off than when the antiquity of Man in the Pampas region was based solely on open air sites, for we have what was always needed, namely, caves with stratified deposits.<sup>5</sup> It is the great merit of Professor Menghin to have enhanced the value of all this evidence, and placed the problems upon a new and more scientific footing. He and his Argentine colleagues are at the head of a promising school of young people, as we have had the opportunity of seeing for ourselves in Buenos Aires; and we wish them all the best of luck in their future work.

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<sup>4</sup> The Basket-maker culture of N. America is dated securely to the first half of the 1st millennium of the Christian Era.

<sup>5</sup> The caves were of course there all the time, waiting to be exploited. Ed.

# How things are made and done

by O. G. S. CRAWFORD\*

A FEW years ago I was talking to a friend of mine in our club. It was obvious that he had, as we say, something on his mind. 'Do you realize', he said, 'that in this technological age we still have no comprehensive history of technology?' I paused for a moment, but before I could speak he had answered my thoughts. 'I don't mean *economic* history—there are plenty of books about that; technology and economics are quite different'. I asked him to expand this. 'Well, take any farm you know. An economic survey of it would cover everything on the farm—field-workers and their wages, birds, animals, dung, crops, carts, ploughs, reaping-machines, dairy utensils. But if it's to be a technological survey you concentrate on the carts and ploughs, how they're made, and how they work. What you are then concerned with is how things on the farm are done and made'. When he thus explained it the distinction became obvious. Seeing what was in his mind I murmured: 'A History of Technology is going to be a big thing'. 'It is', he said, 'for the world is my farm, the human race my farmers and field-workers, and the farm has been a going concern for many millennia'.

My friend was Dr Charles Singer, and it was in his brain that there originated the idea of *A History of Technology* of which the first of five volumes has just been published by the Clarendon Press at Oxford. Associated with him as editors are Dr E. J. Holmyard and Dr A. R. Hall, assisted in the all-important backroom by Dr Jaffé, Mr Garry Thompson and Mrs Donaldson. There is a distinguished list of contributors. The endowment that made the whole work possible came from Imperial Chemical Industries, whose 'Chairman and Directors', says the preface, 'intend this volume and the four that will follow it primarily as a contribution to technical education'. I'm sure that, quoting again, the feelings of the editors 'towards this great corporation for its enlightened patronage will be shared by their readers'. Speaking as a reader I may say that I turned over the pages with ever-increasing pleasure. What pleased me most of all was the mere sight of the book in front of me; for it represents the realization of an ideal—and that is a rare thing in this disillusioning world. An ideal, you will remember, is—again on Oxford authority—a thing conceived as perfect in its kind. No one of course would claim perfection for this book or for any other; but it's quite good enough and there's nothing else like it. Dr Singer has got a load off his mind—or, to be strictly accurate, a fifth of a load, for there are four more volumes to follow. That is no light matter; volume one contains nearly nine hundred pages and is illustrated by six hundred drawings, most of which have been specially prepared for it.

I wonder how many of you realize what it means for an editor to have to handle six hundred illustrations? As an editor myself I've suffered much from illustrations. The greater part of that suffering is caused by bad photographs. You can't improve a bad photograph but you can get a drawing made from it, and if, as here, you get good artists you can ultimately get surprisingly good results even from the most unpromising

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\* This review of *A History of Technology*, edited by Charles Singer and others (Oxford, 1954, £7 7s.), was broadcast in the Third Programme of the B.B.C. on 30 April, 1955, and is printed here without alteration, except that a few sentences, cut out for timing reasons in the broadcast, have been restored. The illustrations are all from the same source, and we wish to express our warmest thanks to the Editors and Publishers for permission to use them, and to the Publishers for special facilities granted. The exact references to the *Hist. Techn.* are given below each.



material. For illustrating an article in an archaeological journal or a chapter in a book like this, drawings are often better than photographs. Some of the drawings in this book are superb examples of good draughtmanship, and some are even more. Not only do they illustrate the text but they also have an artistic excellence of their own. I wish I could say the same about the eight maps lumped together at the beginning; they're just adequate for their purpose, but that's all.

I hope nothing I've said suggests that this book is merely a sort of illustrated encyclopedia of arts and crafts. It's very far from that. Just listen to some of the things it contains.

First of all comes a section headed 'Basic Social Factors' covering such things as the faculty of human skill itself, which was ultimately responsible for all that ensued. Then there are the various processes by which that skill produced results—in other words, how were things first of all invented and then improved? Most of us take all this for granted; but the first tools, the first stones chipped to an edge, had to be *made*—they

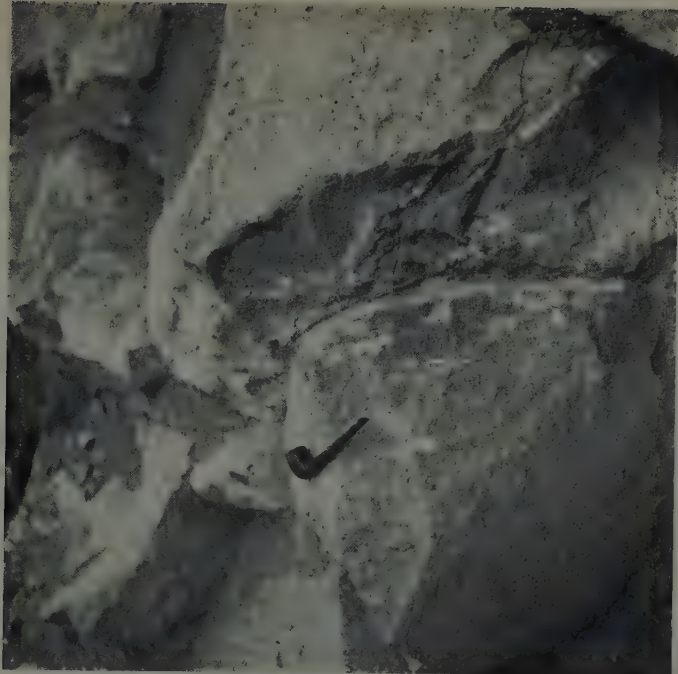


FIG. 1. CART ON A SCARLET WARE VASE FROM SUSA, IRAN; 3RD MILLENNIUM  
(*Hist. Techn.*, I, fig. 125A)

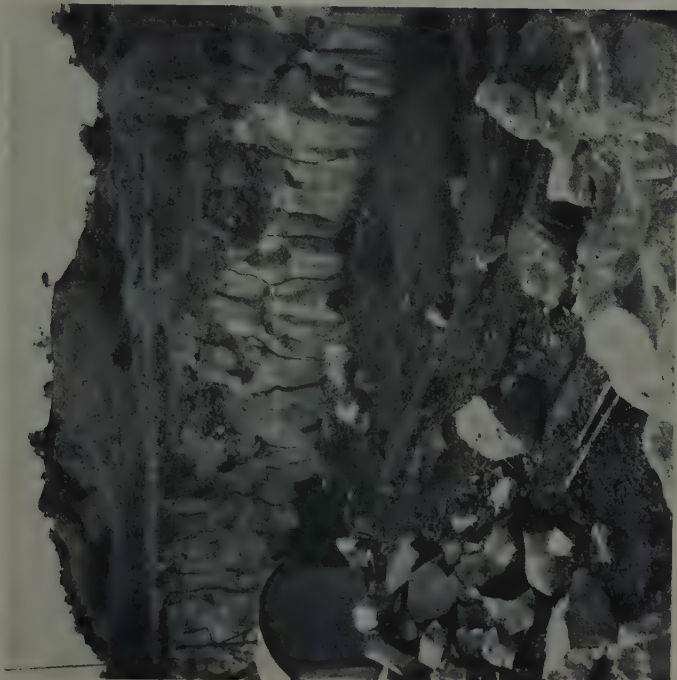
didn't fall from the sky at the feet of our remote ancestor—it took a man's inventive brain to make them.

Then there's the development of speech and language—an essential factor in the story of technology; and the relationship between modes of food-production and early forms of society. Obviously, for instance, the social organization of a group of nomad hunters will be entirely different from that of an agricultural village. Here Professor Gordon Childe, who writes about it, is on his own ground—ground much of which he has made himself.

And so we come at last to the heart of the whole matter, which may be put in the form of a question: Why is the history of technology, that is, of tools and weapons, so important? The answer is, because it largely conditioned the other kind of history which we learnt at school. To take a couple of simple examples: Both the British and the former Turkish Empires were founded on two superior technical achievements—fire-arms and nautical skill. These enabled their possessors to conquer technically less advanced peoples. Here we are on ground that is familiar and is traversed by well-worn tracks leading eastwards; but I'm not following them. On the contrary, I'm going to suggest to you that the new kind of history embodied in this book is a valuable supplement to the old, and that the old cannot be fully understood without it. But I also suggest to you that the new technological history does not supersede political history. What was it, for example, that stopped Xerxes and his hordes of oriental robots from overwhelming the



b. PARTLY DETACHED BLOCK, MAYEN  
*Ph. O.G.S.C.*



a. OLD QUARRY AT MAYEN  
*Ph. O.G.S.C.*

PLATE II



MAYEN QUERN-TYPES

*Top*: Neolithic to Middle Hallstatt

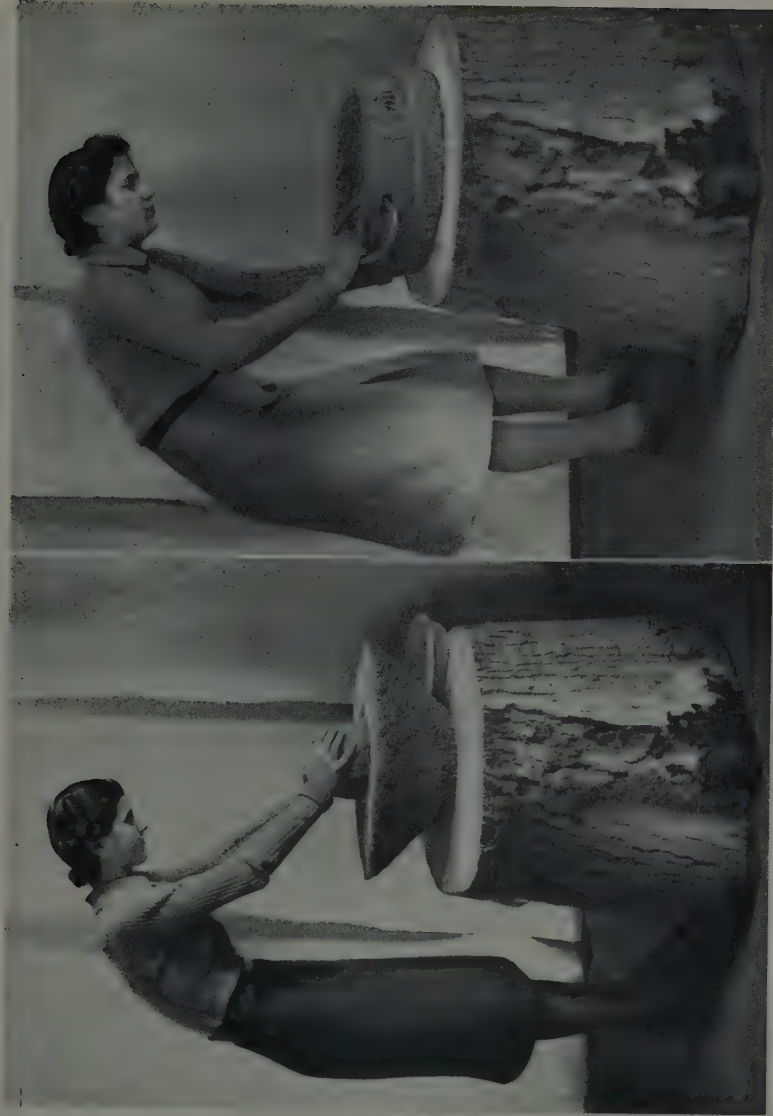
*Middle*: Late Hallstatt

*Bottom*: Napoleon Hats (Early and Middle La Tène)

(Röder, fig. 2)



PLATE III



METHODS OF USE

*Left*: Napolean Hats. *Right*: Oscillating Quern (Röder, fig. 6)

PLATE IV



a. ROMAN WEDGE-HOLES, MAYEN  
(Röder, fig. 18)



b. ROMAN WEDGE-MARKS, MAYEN  
(Röder, fig. 19)

PLATE V



CAVE PAINTINGS FROM RIO DE LAS PINTURAS (COMODORO RIVADAVIA): OLD GROUP  
*Ph. A. de Agostini*





*a.* ROMAN BUILDING NEAR THÉSÉE, TOURAINE. The main block from the south-east



*b.* THE SOUTH-EAST WING

PLATE VII



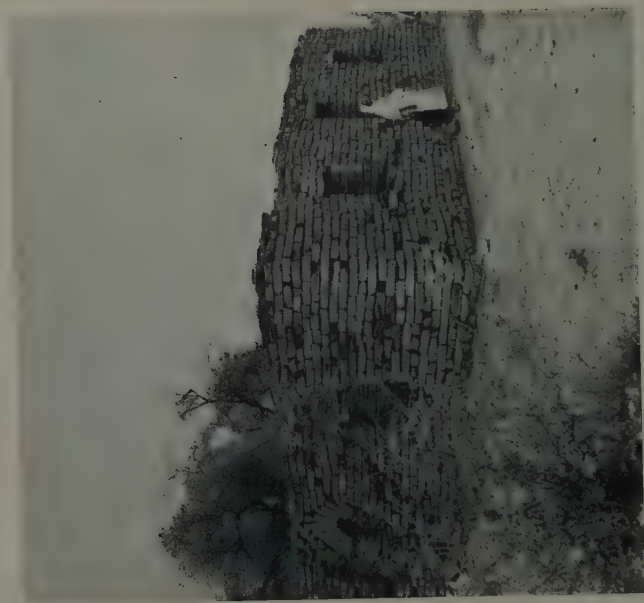
INNER WALL OF PARALLEL PASSAGE, ZIMBABWE RUINS

*Ph. Prof. C. van Riet Lowe*

PLATE VIII



2. MOSQUE OF WAD ANIS ON ISLANG ISLAND



24. MOSQUE OF ABD EL KARIM WAD ACIB ON ISLANG ISLAND



## HOW THINGS ARE MADE AND DONE

Greeks in the 5th century before Christ? It was not technological superiority, but Thermopylae, Salamis and Marathon. So far as armour went, both sides were evenly matched, and of course the Persians far outnumbered the Greeks. Yet it was the stout hearts of the Greeks that won these decisive battles of the world, and prevented Europe from becoming a mere province of Asiatic despotism. But we mustn't forget either that stout hearts alone cannot overcome technical superiority, as the Mahdi found at Omdurman.

This is a book to browse in rather than to read right through. It gives one stimulating ideas. Do we realize for instance that *all* the basic inventions underlying our Western civilization were made in the East? 'Rotary motion', writes Professor Childe, 'in the form we know it in machines and vehicles, was a comparatively recent addition to man's equipment—recent, that is, as measured against the geological time-scale of prehistorians. The potter's wheel and the wheeled vehicle are a bare six thousand years old; the spindle hasn't been used for more than two thousand years longer. Men have been



FIG. 2. PREPARING THE SOIL FOR CROPS  
BY HOING, IN EARLY DYNASTIC EGYPT,  
FROM TOMB OF TI AT SAQQARA  
c. 2400 B.C.

(*Hist. Techn.*, I, p. 375 (tailpiece))

making tools, however, for perhaps some 500,000 years'. The wheel was invented somewhere in the Fertile Crescent between Egypt and the Persian Gulf. Thence knowledge of it spread eastwards and westwards, but it did not reach the New World of America until after Columbus; and its absence from the early literate civilizations which arose there is yet one more proof, if proof still be looked for, of their indigenous origin.

It was also in the same region, on the rim of the Fertile Crescent, that was made one of the greatest of all inventions, the begetter of many others, the invention of agriculture. With the accumulation of capital and the consequent leisure so made possible, there was time for specialization and enough food to keep the non-productive members of the community alive. Not every hour of daylight need be consumed by the perpetual foodquest of the forager and hunter. At last it was possible for some members of the community to take time off all the time, so to speak—in other words, to specialize. Their energies were naturally devoted first to the making of those things that a farming community most needed, such as the hoes which later, tied to a man or a donkey, developed into ploughs; and sickles for reaping the corn; and barns and granaries for storing it; and of course farm-houses for themselves. We mustn't forget that the granaries were of outstanding importance, because in the early days before banks and money, corn was

capital. In view of their subsequent importance there might perhaps have been more said in this book about barns, granaries or corn-stores. This part of it (Part 4) is headed 'Specializing Industries', and apart from agriculture it is concerned with pottery, textiles, basketry and mats—those useful household things that are still properly valued and appreciated in the East—and with buildings.

The stage was now set for the next development—the discovery and utilization of metals, which in turn led to new developments in peace and war. It was now possible to do many things, especially with wood, and such substances as stone, bone and ivory, that hitherto, with non-metallic tools, had either been impossible or too laborious and time-consuming.

Parallel with the growth of specialized industries, went an actual growth in the size of the self-sufficient communities. Some villages became towns; barns became temple granaries and soon required a special staff for their maintenance. But they required more than mere supervision; their contents had to be checked and docketed, and there had to be some record other than memory of what was due. Consignments had to be marked and sealed. Out of all these needs arose the momentous invention of writing. 'The earliest known examples that can be called writing,' says Professor S. H. Hooke,



FIG. 3. PLOUGHING AND HOEING IN EGYPT, FROM A TOMB AT BENI HASAN, c. 1900 B.C.  
(*Hist. Techn.*, 1, fig. 43)

'come to us in the form of clay tablets from the temple of Inanna at Erech. On these tablets are inscribed signs, assumed to be numerals, and pictures of objects, such as heads of animals, birds, fishes, plants, domestic utensils and parts of the human body'. These signs are in fact symbolic pictures. 'The motive which brought about this invention of writing was economic', says Professor Hooke. I am stressing this point because I've heard people talking quite wildly about it, imagining that writing was invented to record lofty ideas about the universe or beautiful poems or historical events. Nothing could be further from the truth. The origin of writing was revealed by archaeologists—German archaeologists—during the nineteen-twenties. There isn't the slightest doubt or mystery about it; writing was invented in Mesopotamia about the middle of the 4th millennium before Christ, and in the *History of Technology* there is a picture of one of the first essays at it. 'These earliest tablets (says Professor Hooke) are records, kept by the priests, of the temple incomes. A tablet containing a picture of a cow's head, an ear of corn, or a fish, together with round dots taken to be numerals, would be a memorandum of so many head of cattle, so much grain or so much fish, due to the temple from some individual or village community. A similar tablet with a hole pierced through the centre perhaps records dues paid; in other words, it is a receipted bill'.

Writing, then, was invented by priests for what we should call 'business purposes'. The priests stored and guarded much of the community's capital—the surplus product of its labour over and above subsistence level; and that capital was then locked up and became incapable of stimulating further inventiveness. Hence, as Professor Childe told

## HOW THINGS ARE MADE AND DONE

us long ago, the sudden drying up of that stream of new inventions that followed in the wake of the invention of agriculture.

This book is also concerned with what we call the 'diffusion of culture'—how technical achievements became known and adopted in regions far from the source of their origin. There were already permanent settlements of food-producing communities—of farmers—at Jarmo and elsewhere round the foot-hills of Iraq in 5000 B.C., a date securely established by three independent radiocarbon tests. The earliest settlement at Jericho, now being excavated by Miss Kenyon, is unlikely to be much later and may well be earlier. In the country between these two places wild cereals are found, and it's there that they were first taken and planted and developed for human use. The knowledge of

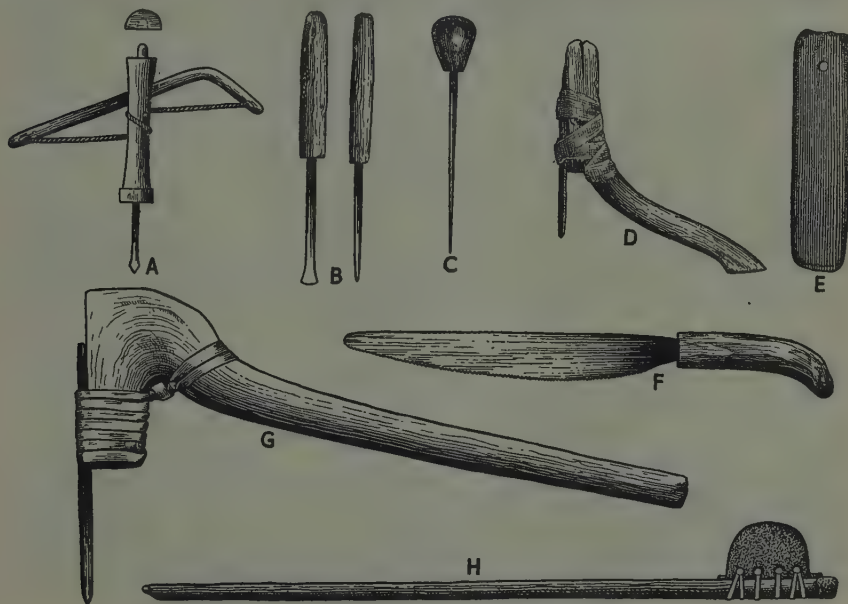


FIG. 4. EGYPTIAN WOOD-WORKING TOOLS, c. 1200 B.C.  
(*Hist. Techn.*, 1, fig. 487)

the new technique of living—for it was nothing less—soon spread outwards in all directions. It's a safe guess that the spread was helped, if not actually initiated, by population pressure. Younger sons would hive off and set up new farms in the sparsely peopled lands of the foragers and hunters. By some such stages agriculture—and the seed of corn too—must have travelled southwards up the Nile Valley and northwestwards up the Danube into Central Europe. I would suggest that there must also have been a westward expansion of food-producers across the Mediterranean, perhaps by the process known as 'island-hopping'. But here we touch upon an almost unexplored field of knowledge, bristling with problems and with difficulties of approach. How and when did a knowledge of food-production reach Crete and Greece, Sicily and Spain, and the various islands in between? We know from Mr Dikaïos's excavations at Khirokhitia that it reached Cyprus in the pre-pottery Neolithic period.



Professor F. E. Zeuner hints at the possible existence of a Far Eastern centre of cultivation at least as old as the western one in the Fertile Crescent. One thinks at once of China—that great and archaeologically unexplored land. Apart from Choukoutien and some Swedish excavations there has been (until quite recently) no scientific excavation there, and it has been the victim of treasure-hunters, collectors and art-museums.

All these different movements and migrations involve methods of transport, and, on land, some sort of trackways. A short section of the book is concerned with land transport, boats and ships. Evidence here is hard to come by. To take an extreme case: How can the excavation of an island site reveal to us, how the people got there? There



FIG. 5. THE 'CLERKS' OF TIGLATH-PILESER COUNTING THE SPOILS OF WAR,  
CENTRAL PALACE AT NIMRUD, IRAQ, c. 800 B.C.

(*Hist. Techn.*, 1, fig. 32)

are many ways of crossing the sea; short distances may even be crossed by swimming, as did the Guanches in the Canary Islands.

You will now, I hope, have got some inkling of the importance of this History of Technology, and of the width of its scope. Knowing something of the design of the whole work perhaps in conclusion I may anticipate a little on the later volumes. When they're all published, what sort of panorama of human history shall we have? We shall, I think, see the whole of that history falling into three periods, the first being immensely longer than both the other two combined. The first period is the Age of Foraging, Hunting and Fishing; it lasted for something like half a million years. During that period the physical type of man became fixed, for good or ill, as *Homo Sapiens*. Then, not long after the end of the Ice Age and not later than about 5000 B.C., food production was invented and the Age of Farming began. After the first flush of new inventions which followed it,

## HOW THINGS ARE MADE AND DONE

there was a lull of more than six millennia which passed without registering any invention of comparable importance. The self-sufficient village communities of the Fertile Crescent differed hardly at all in essentials from those of, say, Elizabethan England. Both must have used human and animal power for tilling the fields, lamps or candles for lighting and wood or dung fires for cooking. But the village of today differs from the Elizabethan one in one essential feature; petrol, not men or animals drives the ploughs and reaping-machines, and electricity lights the house and cooks the food. The Age of Farming has given way to the Age of Power Production, the third great period, in which we are living today. It is fashionable for journalists to talk about the Atomic Age, as if it were yet another, a fourth, period. But even when atomic power is successfully harnessed to industry the result will merely be another form of power production not differing in essentials from electricity.

You will now see how right Dr Singer was when he said that the world was his farm! He has taken on a big job with a courage and an enthusiasm that belie his years; and the first and probably most difficult volume is off his hands. I say 'difficult' because the evidence, nearly all of it archaeological, is for that very reason defective in parts; that cannot be helped. We should rather be grateful that archaeology has made it possible to say so much about the prehistoric ages. The majority of the six hundred drawings in this first volume are of things dug up by archaeologists during the last hundred years, so it would have been impossible to write this book at all in 1854. As an archaeologist I am of course delighted to see the fruits of archaeology put to such good use in this splendidly conceived and admirably executed work of synthesis, and look forward eagerly to the appearance of the next volume.



FIG. 6. GOLD HAWK'S HEAD FROM HIERAKONPOLIS, EGYPT, c. 2400 B.C.  
(*Hist. Techn.*, 1, fig. 432)

# The Ruin at Thésée

by R. M. BUTLER

THE aim of this note is to bring to the attention of English readers a Roman building of large size, whose impressive ruins are well preserved, but whose purpose is uncertain and existence little known.

The Roman road from Tours to Bourges ran along the north bank of the river Cher and for much of its length is followed by the modern N76. Two settlements or posting-stations lying on this road and marked on the Peutinger map under the names *Gabris* and *Tasciaca* can be located at the villages of Gièvres and Thésée respectively. At this latter place, otherwise called Thézée or Tézée, 31 miles east of Tours, the main road, following the course of the Roman route, runs close beside the river, with the railway line separating them and low, bare hills rising immediately to the north. The towns of Montrichard to the west and St. Aignan on the east are respectively 4 and 5 miles away, and the village lies in the Loir-et-Cher département.

Some 300 yards to the west of Thésée village there is a recess in the hills bordering the road, which here runs north-westward towards Montrichard. At this point, 250 feet north of N76, stand the ruins of a large rectangular building which can be recognized as Roman by the masonry and tile levelling courses of its walls, which rise to a height of 27 feet. The structure is of simple plan. It consists of a rectangular hall, orientated roughly east-west, and measuring 132 feet (40 m.) long and 43 feet (13 m.) wide, to which are attached three smaller rooms<sup>1</sup>. There were two main entrances, apparently of similar form—one in the centre of the south wall and the other in the centre of the west end (FIG. 1). Although their arched heads have long since collapsed, sufficient remains to show that these doorways were each 10 feet wide and some 20 feet high to the crown of the arches, which, like all the others in the building, had voussoirs of stone alternating with double tiles, but were surrounded by an additional outer frame of tiles. In the centre of the east wall, but only 3 feet above the ground, there was a round-headed window 2 feet high and 1 foot wide, and there also seems to have been a doorway at the south end of this wall, about 5 feet high and 3 feet wide, though modern reconstruction of this part of the wall makes this uncertain.

Apart from the two large doorways and the tiny east loophole, the hall was lit by a series of windows in the long sides with their sills 22 feet above the ground. Each of these had a shallow-arched head of stone and tiles, but its jambs were splayed inwards,

<sup>1</sup> The first publication was by A. de Caumont in *Cours d'Archéologie monumentale*, III, 1838, 134 ff. and *Atlas du Cours*, Pl. xxxviii. There are accounts based on De Caumont in L. Saussaye. *Mémoire sur les antiquités de la Sologne blesoise*. Blois, 1844, 21, and in *Bulletin Monumental*, 1864, 165 ff. C. Roach Smith's description in *Collectanea Antiqua*, IV, London, 1857, 1 ff., appears to be independent, and is illustrated by the best of the early drawings of the site. When compared with photographs, however, these are seen to be inexact in details. Grenier's account in *Manuel d'Archéologie gallo-romaine*, II, Paris, 1934, 205 ff., is based on De Caumont, strangely ignoring the best publication of all, that by Lesueur in *Bulletin Monumental*, LXXXVI, 1927, 129 ff. The plan illustrating the present article follows Lesueur's for the main block, but the dimensions of the vanished courtyard walls, taken from De Caumont, may not be completely accurate. The photographs were taken by the writer, who examined the ruins in September, 1953.



## THE RUIN AT THÉSÉE

so that, although they were internally 4 feet wide and  $3\frac{1}{2}$  feet high, the external openings were only 2 feet square. These 'clerestory' windows were 10 feet apart and so arranged that there were ten in the north wall and eleven in the south one, the extra window being

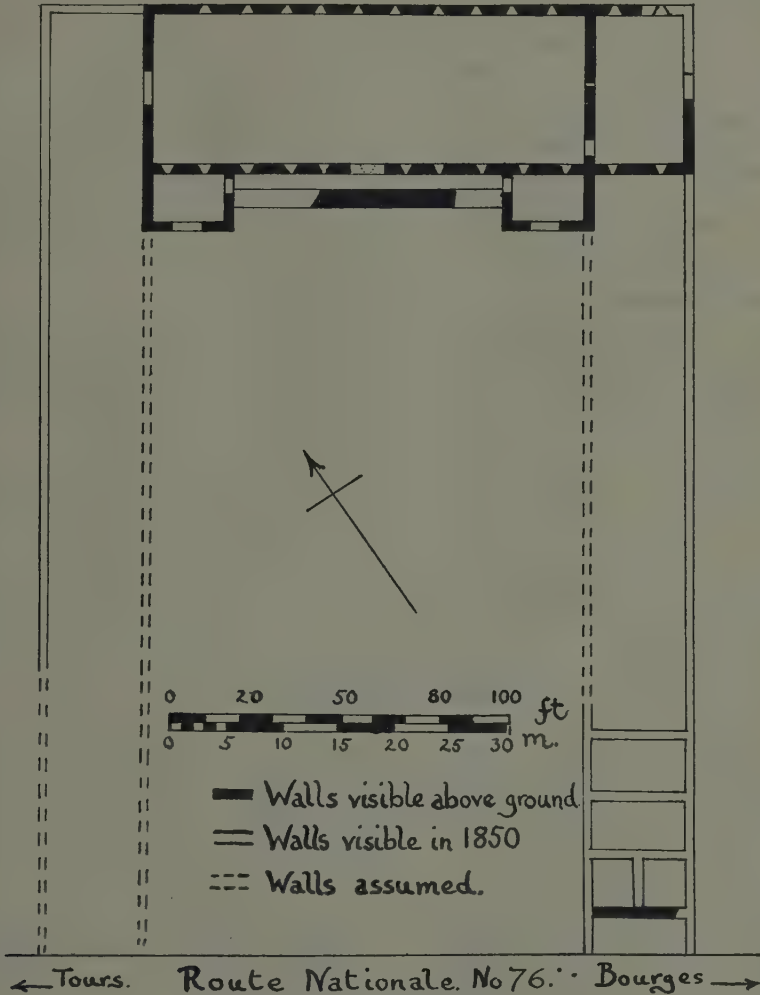


FIG. 1. PLAN OF ROMAN BUILDING NEAR THÉSÉE, EAST OF TOURS

above the centre of the south entrance. There is no sign in the existing walls that there were any similar apertures in the short sides of the building.

The main hall was continued eastward by a room of the same width and height but only 26 feet in length. Part of a tile and stone arch still remains in the centre of the east

wall of this apartment. This belonged to a doorway somewhat smaller—it measured 18 by 8 feet—than those of the hall, and there may have been a second, but much lower, entrance on the south side, but the wall there is broken away in a large opening. Two upper windows of exactly the same type as those described remain in each of the north and south walls.

At either end of the south front of the larger room there is attached a small annexe or pavilion. The eastern one is the better preserved: it is 26 feet long and projects 16 feet (PLATE VI, B). The only means of access is by a doorway in the west wall. This has been repaired in modern times, but was originally square-headed, measuring 7 by 4 feet, and had a relieving arch above. A gap in the centre of the south wall marks the position of a round-headed window, one side of which remains. The walls of the western pavilion only stand 4 feet high, and De Caumont described it as being but two-thirds the size of the other ( $11\frac{1}{2}$  by  $7\frac{1}{2}$  feet), but Lesueur's plan shows them as of equal size, and the writer's impressions confirm this view. The western room apparently had a door and window similar to those of the other annexe and like it would only have stood about 14 feet high.

Between these two wings and a few feet in front of the south wall a length of very thick walling, projecting slightly above the ground but almost concealed by brambles, seems to indicate that there was a terrace or a flight of steps in front of the main entrance. The ground level inside the hall and its wings is 2–3 feet higher than that of the area to the south and, since the remains of stone paving in the east wing suggest that this was the ancient floor level, a stepped approach would have been necessary.

A short length of wall of the same type as that in the main block stands about 220 feet to the south of it. This is now the only indication that the great hall and its annexes originally formed part of a larger complex of buildings. A century ago further walls, standing 2 feet high, could be traced, and these showed that the visible ruins had formed the north side of a rectangular courtyard measuring 200 by about 250 feet. The isolated fragment of walling had been the south wall of one of a row of small rooms along the eastern side of the court. Three of these could once be distinguished. Further foundations and finds of pottery on the far side of the main road indicated that other buildings, perhaps connected with the ruins, had extended to the river bank.

The surviving walls are of uniform appearance, but although their construction makes a Roman origin certain, there are no features to indicate any particular period. These walls are 2 feet thick, built of small ashlar blocks, generally 4 by 5–6 inches and laid in horizontal courses. In several places, however, thinner slabs are arranged obliquely to make a herring-bone pattern. In the main hall, the southern wings and the isolated stretch of wall, double tile courses running through the whole thickness of the walls occur at 3–4 foot intervals. Four of these are visible, and they maintain a notably level course all round the building. Similar tiles appear in the arches of doorways and windows and in intermediate courses ornamenting or strengthening the outer angles of the wings. Those who have studied the ruins have remarked that the eastern room and the two pavilions are not bonded into the walls of the principal apartment but abut against it, and this is especially noticeable in the case of the eastern annexe, since the horizontal tile courses do not continue across its south wall. Its angles are, however, strengthened with tiles like those of the wings, and on the north side the upper part of the wall above the level of the windows is bonded in with the adjacent northern wall of the hall. Apart from the absence of tile courses, the masonry is indistinguishable from that used in the rest of the block.

## THE RUIN AT THÉSÉE

The rows of putlog holes form a prominent feature of the walls (PLATE VI, A and B) and generally appear just above the tile courses, showing that, as in the town walls of the Late Empire, these were intended quite as much to level off any irregularities in the stage of walling below them and to offer a firm basis for the scaffold poles as to bond together the stonework or to give a decorative effect. The putlog holes penetrate the whole thickness of the walls and in the places where there is no tile course immediately below the opening they are covered by two tiles or a small slab to prevent any possibility of the courses above subsiding.

Now that the surviving remains have been described in detail it is possible to deduce the original form and appearance of the structure to which they belong, and hence to speculate on its purpose and date. The major portion of the ruins at Thésée belonged to a large hall with lofty arched entrances in two sides and rows of small windows high up in the long walls. It seems likely that when first erected these walls were not much higher than at present, and that the hall was covered by a timber roof in a single span. A division into aisles by columns is made most unlikely by the fact that the windows in the two side walls are not opposite each other and by the lack of any trace of gables at the ends. This latter point suggests that the tiled roof was hipped and sloped back at east and west to a central ridge. It is uncertain whether the east room and the two wings were later additions or were an afterthought of the original designer, but in view of the uniformity of the construction the latter view seems more likely. At all events it is clear that the eastern annexe was under the same roof as the hall. The wings were probably covered by pyramidal roofs which did not rise above the level of the uppermost tile course of the wall behind them, but it is possible that the larger size of four putlog holes just below the windows above the east pavilion is not accidental, but that these were in fact intended to hold the ends of the beams of its roof.

The south portal of the hall was apparently approached by a flight of shallow stairs or had before it a low platform—the lack of traces of a lean-to roof makes a portico unlikely—and this façade clearly formed the dominant feature of a large rectangular court extending southwards to the main road, where it was perhaps entered by a gate building. There was a range of humbler rooms on at least one side of this court and whatever its purpose it is clear that this group of buildings must have been among the principal landmarks of the Roman *Tasciaca*.

There have been several conjectures on the purpose of this structure, and it has been variously described as a fort, a villa, and a posting-station. The first suggestion is manifestly absurd, and, although some resemblance can be seen between the plan of Thésée and that of a large mansion with a verandah between projecting wings, such as Cologne-Mungersdorf<sup>2</sup>, the great size of the hall makes its use as part of a private house unlikely. No other *mansio* seems to be known with buildings of such an impressive size, and the main room would seem to be ill-suited for a common hall in which travellers and bodies of troops passed the night, or for a large granary, as Roach Smith suggested. The orientation, clerestory lighting, and east window would support the theory that this was the church of an early monastery, for which the adjoining buildings would be suited, but again the scale and elaboration of the main block are against such an attribution and, although the site is in the area where St. Martin was most active, some tradition would be expected to have survived were this a monastic foundation of that period (c. A.D. 380), especially as its grandeur would have excited comment.

<sup>2</sup> See Grenier. *Manuel*, II, 814 and figs. 275-9 and 291-3 where plans are given of a type of villa with a central hall, wings, and a portico, showing some resemblances in plan and restored elevation to the Thésée building, though on a much smaller scale.



To the writer the most likely explanation of this impressive structure—one apparently not previously suggested—is that these are the ruins of the forum of *Tasciaca*, admittedly unusually elaborate for such an unimportant place. The main hall could thus reasonably be recognized as the basilica with a smaller court-room at its eastern end. This explanation, however, can only be offered with hesitation since there seem to be no exact parallels for a basilica of this type, at least none in Britain or Gaul. Of those known nearly all seem to have had an aisled plan, although those at Martigny and Alesia<sup>3</sup> were of much the same size as the Thésée hall. Somewhat similar projecting wings existed at Leicester<sup>4</sup>, though if the building there was indeed the main basilica they were probably attached to its rear wall, and a terrace between these wings would be quite natural in such an edifice. The small rooms on the east side of the courtyard at Thésée would be the shops which normally surrounded a forum, and the position of the whole group of buildings adjoining a main road is what would be expected if such were its purpose.

In view of the lack of any excavations nothing certain is known of the date of the building. It has been suggested that the tile courses are a sign of a 4th or 5th century date, but very similar masonry can be seen in the ruins of the 1st century forum at Bavai<sup>5</sup>, where herring-bone coursing occurs together with frequent tile levelling courses and arches with stone voussoirs alternating with double tiles. Nearer at hand close parallels to the methods of construction used at Thésée can be found in buildings of such varying dates as the Cluny baths at Paris, the aqueduct at Luynes, and the town walls of Tours and Le Mans. The masonry can thus offer no sure indication for the date of erection, nor is the splendid state of preservation much help for this purpose, since there are plenty of Roman buildings in Gaul built in the 1st and 2nd centuries which are in an equally fine condition, although many of them, such as the theatre at Orange or the great gatehouse of Trier, owe their survival to their incorporation in later castles or churches. The lofty ruins of the baths at Valognes and the still larger temple of Vesunna at Périgueux still survive, however, although, like the Thésée monument, they have never served any purpose in modern times. The most likely date for the building, if it was a forum and basilica, is the mid-2nd century and the memorable reign of Hadrian. Whatever their date, the ruins of the great hall and its annexes are an impressive survival from the Roman period in Gaul and a problem for students of that epoch.

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<sup>3</sup> For Martigny see F. Staehelin, *Die Schweiz in Römischer Zeit*, 3rd ed., Basle, 1948, 159 ff. and 618 ff.; for Alesia see J. Toutain, *Pro Alesia*, v, 49 ff.

<sup>4</sup> K. Kenyon, *Excavations at the Jewry Wall Site*. Leicester. Oxford, 1948, 19 ff.

<sup>5</sup> H. Biévelet, *Gallia* I, 1943, 159 ff.; v, 1948, 301 ff.

# The dating of the Zimbabwe Ruins

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LIKE many other spectacular monuments, Zimbabwe Ruins has attracted more attention through speculation than through research. Many of the so-called 'archaeological excavations' in the early days were little more than licensed treasure hunts, whilst the amount of unauthorized fossicking for gold which went on until 1902 (when the ruins were protected by legislation) destroyed acres of archaeological deposits. Two people only have published reports of widespread excavations on scientific lines at Zimbabwe: the late Dr D. Randall-MacIver, who dug there for a few weeks in 1905 (1) and Miss G. Caton-Thompson, whose careful work during the dry season of 1929 forms the basis of our present archaeological knowledge of this site (2). Considering the great area involved—about 100 acres of actual buildings—and the obvious importance of the site, surprisingly little is really known about it. For the past 18 years the site has been under the protection of the Southern Rhodesian Monuments Commission and excavation has been very wisely discouraged, although on the other hand the exploration and excavation of other sites has proceeded apace. Both MacIver and Caton-Thompson agreed that the building was African. MacIver dated the 'Temple' (the principal single structure) as 'not earlier than the 14th or 15th century', but admitted that an earlier settlement prior to the present buildings was a possibility. Miss Caton-Thompson gave as her earliest date for the whole complex the 9th century A.D., or possibly a little earlier, and also considered a pre-ruin culture not unlikely. Wieschhoff after reviewing all the preceding evidence and undertaking excavations at a number of other sites found himself fully in agreement with MacIver as regards dating (3) whilst the present writer, with still more field evidence from other Rhodesian sites, felt inclined to shorten Wieschhoff's dating still more (4).

There the matter rested until, in June 1950, the Warden of the Ruins, Mr S. D. Sandes, noticed some pieces of wood wedged in the top of a drain. These were examined *in situ* by a number of Rhodesian and South African archaeologists and in October 1950 the wall was opened by the Southern Rhodesian Public Works Department in the presence of the Inspector of Monuments, Mr K. R. Robinson. It was then clear that the wood, of which two short logs remained, had been inserted when the wall was built to carry the weight of the wall where it crossed an opening about 3 feet wide in the wall foundations. They were in effect wooden lintels carrying the wall above a drain.

It was hoped that a C-14 test on these timbers would provide an objective method of determining the age of Zimbabwe Ruins. Dr W. F. Libby in Chicago and Professor F. E. Zeuner in London, kindly accepted specimens of these timbers and submitted them to a series of C-14 tests. The results of these tests were as follows, tests numbered C being performed in Chicago and that numbered GL in London:

# ANTIQUITY

Zimbabwe I	C613 (5)	1415 ± 160	1361 ± 120 B.P. (1952)
		1344 ± 160	= A.D. 591 (± 120)
		1271 ± 260	
Zimbabwe II	C917 (6)	1506 ± 350	1252 ± 92 B.P. (1954)
	GL19 (7)	1240 ± 80	= A.D. 702 (± 92)

The last two are the most remarkable. Two tests, made in laboratories as far separated as Chicago and London, by independent operators using different techniques, have yielded results which agree within the limits of experimental error. These two results seem to dispose of any lingering doubts which archaeologists may entertain concerning the validity of the C-14 method of dating.

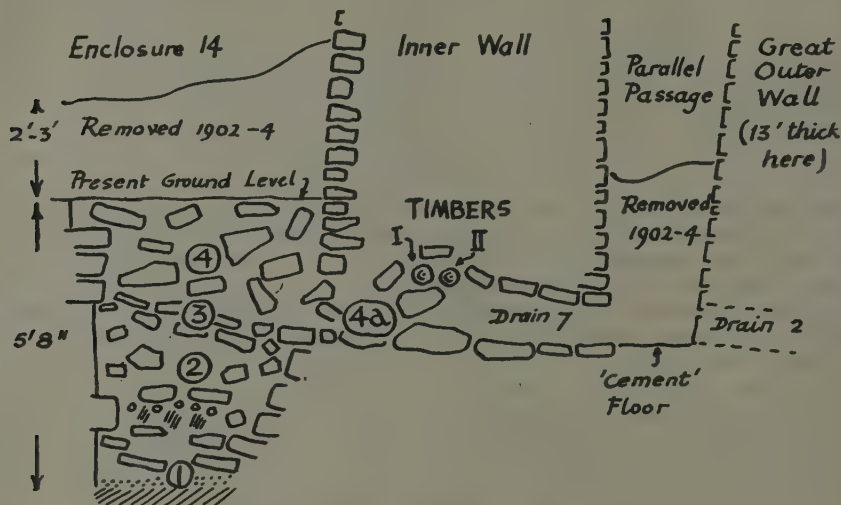


FIG. 1. SECTION THROUGH SUMP AND DRAIN (Scale  $\frac{1}{80}$ )

We therefore know that the trees from which these logs were cut died about 1200 or 1300 years ago. Both timbers have been identified by a number of Africans as *ubande* and the Southern Rhodesian Forestry Department in confirming this gives the botanical name as *Spirostachys africana* Sond. It is exceptionally hard, is ant-proof and decays extremely slowly; it has no economic value nowadays, being too hard to work commercially, its main functions being as a native cosmetic—it has a sandalwood smell—or medicinally. It does not grow at Zimbabwe nowadays, the nearest known occurrence being 30 miles away.

When removing the timbers Mr Robinson took the opportunity of excavating on the inner side of the wall and the following notes and section are taken from this report.

(a) In Enclosure 14 on White's plan (8), there was a sump 6 feet by 3 feet enclosed by three walls beneath ground level and the main Inner Wall. The floor of the sump was composed of a rough pavement of stones and sloped towards the drain.



## THE DATING OF THE ZIMBABWE RUINS

### (b) Stratification in Enclosure 14 was :

- 4a. Drain Fill : hard silted earth. 18-24 inches.
4. Sump fill : loose ochreous earth with wall stones, fragments of bone, sherds, ash, hut daga (i.e. mud wall).
3. Floor of Sump, 4-6 inches.
2. Debris : Wall stones, earth, old hut daga, sherds some carbonized wood (piece of post). 40-45 inches.
1. White Ash. 2 inches.
0. Granite sand (Natural). (5 feet 8 inches from present surface).

### (c) The contents of the various levels were sifted as removed and finds were :

- 4a. Drain Fill : nothing.
4. Sump Fill : a green glass biconical wound bead (cf. Caton-Thompson's Type 4h), 2 gold and 1 copper beads ; fragments of soapstone bowl and other objects ; sherds.
2. Debris below sump : soapstone fragments and sherds (9).

The bead is presumably of Arab origin as it is not uncommon on the beaches of Pemba and Zanzibar Islands. Kirkman records one from Gedi in a 15th century level (10). The sherds were not particularly helpful being body sherds of Caton-Thompson's Class B ; but it is significant that not a single Class A sherd was found, although looked for. Hall admits the removal of from two to three feet of deposit from within Enclosure 14 in 1902-4. This deposit on Hall's evidence contained at least two smoothed 'cement' floors and the presence of trimmed walling stones in stratum 4 suggests that the sump may at one time have opened at a higher level than it does today. It is scarcely surprising that the sump contains a miscellaneous collection of items.

The presence of wall stones in stratum 2—the fill under the pavement—and the presence of charred logs and fragments from the walls of mud huts shows that there were stone walls and pole-and-daga huts before the drain was built, and that Class B pottery was used at that time.

PLATE VII (from a photograph taken in 1948 by Professor van Riet Lowe) shows the construction of the wall at a point about 120 feet north-west of the drain. It is typical of all free-standing walls at Zimbabwe and other Rhodesian ruins. It had been built dry with faces of selected blocks and a rubble core consisting mainly of large angular blocks. The facing blocks are not tied into the rubble nor is there any sign of through courses. Schofield commented on this construction nearly 30 years ago (11) and more recently van Riet Lowe has drawn attention to these and other evidences of lack of any tradition of masonry (12) ; both these writers although well known for their archaeological work received their original training as architects and civil engineers respectively, and their comments on Zimbabwe constructional details are therefore the more valuable. It is, however, a matter of observation that the angle of repose of the rubble filling is a very high one—in most cases 60-70°, but sometimes as high as 80°. All these large walls are battered back at angles varying between 70° and 80°, so that they lean against the rubble core at an angle very little greater than its own angle of repose. It would seem therefore that this form of construction is probably much more stable than appears at first sight.

A careful re-reading of all the accounts of digging in the ' Temple ' (1, 11, 13, 14) shows that the base of the inner wall explored by MacIver stands at a much higher level

than that of the wall which we are now considering. Since none of these walls have ever been built on conventional foundations but merely on a roughly levelled surface, we may properly conclude that MacIver's wall, which on intrinsic grounds he considered 'one of the oldest in the Temple', is in fact considerably later than the one under consideration. From imported china and glass MacIver dated his wall (and by inference, the whole of the Temple) as 14th or 15th century. Our own wall must be older than this and consequently the date provided by the radiocarbon tests does not seem unreasonable.

Archaeologically minded architects such as Douslin and Schofield who actually worked at Zimbabwe have doubted the ability of the building to stand for many centuries and have denied that it could possibly be as old as the 3000 years or so claimed by Hall. The radiocarbon date is considerably earlier than Schofield was at one time prepared to accept, but it is nothing like the age suggested by Hall, and probably the arguments based on poor construction can now be discounted.

There are therefore good reasons for accepting the C-14 date as the date of the wall. Against this is the fact that the contents of Stratum 2 show that reconstruction of stone and wooden buildings took place before the drain and superjacent walls were built: the possibility of the timber having been reused is thus not ruled out completely.

There is a general disinclination amongst archaeologists working in Southern and Eastern Africa to accept the obvious interpretation of this unquestionable date. The matter has been widely discussed (usually by correspondence, for distances are vast and archaeologists few), but very little has been published despite Press publicity given to Dr Libby's first determinations. Not only have Schofield (15) and Wieschhoff (3) independently propounded a short chronology but this has the support of the history of Bantu peoples as interpreted from genealogies. It is remarkable that even on the East Coast Kirkman has found little that is older than 12th century A.D. (10, p. 14), although there is historical evidence for Arab settlement on the coast at a much earlier date.

It would seem therefore that archaeology does not, in the present state of our knowledge, provide sufficient clues to enable us to reconstruct even the outlines of a history of this part of Africa.

It is, perhaps, not out of place to draw attention to a fragment of legend, collected many years ago by Mr J. Blake-Thompson but not yet published, to the effect that long ago there was a building on a hill to the east of the present ruins which was used as a shrine connected with some old cult. This building was destroyed during the upheavals connected with the introduction of the present *Mwari* cult, and the present buildings were erected to house the worship of Mwari. Possibly this fragment of folklore may provide a solution of the archaeological *impasse* which the indisputable C-14 dating has caused but, since we have no clue to the site of the old shrine, it seems unlikely that we shall be able to confirm the suggestion by direct archaeological methods, although some archaeological pointers to a change in religion have been noted in the past (4, p. 104).

Whether one accepts the obvious interpretation that the wood dates the building directly or the locally more accepted interpretation that the wood has been reused, one cannot escape the conclusion that, through the efforts of scientists in Chicago and London, we now possess a firm date that the Zimbabwe site was occupied as long ago as about A.D. 700.

Our thanks are due to Dr Libby and Professor Zeuner for their work in connection with the determination of the radiocarbon dates, to Professor van Riet Lowe for permitting the use of his photograph, and to the Monuments Commission of Southern Rhodesia for allowing publication of Mr Robinson's report.

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## Notes and News

### SPOUTED VESSELS FROM NĀVDĀ ṬOLI (MADHYA BHARAT) AND IRAN

While studying the painted pottery from the Chalcolithic layers at Nāvḍā-Ṭoli, opposite Maheshwar, in Madhya Bharat (Central India), the writer was struck by the occurrence of a certain characteristic motif, consisting of dancing human figures on a white-slipped pottery. Since this motif was also familiar in Western Asia in proto-historic times, it was thought desirable to draw the attention of scholars to the similarity in designs with a view to eliciting further information with regard to shapes on which these motives are depicted (1).

Further study of the Nāvḍā Ṭoli pottery has produced yet more points of similarity in decoration as well as in types of pottery with designs and shapes from Iran, which may turn out to be very significant as establishing culture contacts between India and Western Asia during the proto-historic periods. Hence it is sought to publish this evidence at an early date.

This consists of Channel—or Cut-away-spouted—vessels. In all there are 15 sherds, fourteen of which come from the middle layers of the proto-historic—pre-Northern Black Polished—pottery deposits of Mounds I and III at Nāvḍā Ṭoli, while one was found on the surface. These sherds suggest the use of two very specialized types of pottery vessels.

(i) A vessel—probably a cup or bowl—with a Channel-or-Cut-away-spout (a).

(ii) A cup or bowl with a small V-shaped projection, as in a milk jug (c).

Unfortunately the full shape of the vessels cannot be gauged, but from the nature of the five spouts (a, d, e, f, g), and the three sherds where a part of the body of the vessel, together with the beginning of the spout has survived, it can be definitely said that—whatever the full shape of the vessel may be—the spout projected from the edge of the rim as in the reconstruction shown in the figure (a). Eight other sherds suggest the shape illustrated in (c), and in every case the geometric design (a, c, d, f, g), or a leaf-like design (e) is painted in black over a fine smooth brown, red, or chocolate slip, the fabric being of medium thickness having a dusty core.

Now, vessels with such designs and spouts, possibly for libation, do not occur in the Harappa Culture, or in any other proto-historic or early historic culture of India, barring one possible exception, viz., a cut or 'beak'—spouted jug (2), excavated from a stone circle in the Perumal Hills, Kodaikanal, S. India. This vessel, though of a red colour, is not painted. The date of this and other associated vessels cannot be accurately fixed, as no iron work is reported by Aiyappan who excavated it in 1940. But Aiyappan even then pointed out that such a vessel was unknown in India, but resembled those from Iran, where their history can be taken back to at least 1500 to 2000 B.C., and suggested that 'the form may be a survival of an Asiatic type of pottery'. Thus a long gap both of time and space seems to separate the South Indian and Iranian Channelled-spouted vessels.

But this would not be the case with vessels from Nāvḍā Ṭoli. The Chalcolithic layers can be, stratigraphically as well as culturally, dated to a period between 500 B.C.—1,000 B.C., if not earlier. Now whatever the exact date of this Nāvḍā Ṭoli Culture may be, since Frankfort wrote in 1932 (3) a number of sites in Iran and Western Asia (4) have been excavated, some of which have yielded Channel-spouted pots; the closest similarity both in decorative design as well as in the form of the vessel being found in the Channel-spouted pots from Necropole B of Sialk (5) and in similar examples from Tépé Giyan (6).



**PAINTED CHANNEL SPOUT POTTERY TYPES  
 NAVDATOLI- CENTRAL INDIA & SIALK - IRAN**

For the sake of easy comparison one of the former is reproduced here (b). A criss-cross design in black between two parallel running bands decorates the spout and the portion just below the rim in both the vessels ; the vessel from Sialk has, however, in addition an 'S'-like design on the criss-cross. With regard to the form, the spouts of both the vessels seem to be identical ; they differ, however, in respect of the forms of the body. Here it may be said that we do not know the entire shape or all the types of Channel-spouted Ware from Nāvḍā Ṭoli. Iran, however, had also had a Channelled-spout in handleless cups. These are also met with in alabaster and common stone in Hissar III c, and the form first seems to appear in Hissar III B (7).

Now the cemetery B at Sialk is dated to a period between 800 B.C. to 1000 B.C., a date which we have already assigned to the Painted Pottery and Microlithic Blade Culture at Nāvḍā Ṭoli and Maheshwar, because the deposits of the latter just underlie those in which are found the Northern Black Polished Ware, and a Silver Punch Marked Coin.

In the absence of evidence of Channel- or beaked-spouted vessels from Baluchistan, and the Indo-Iranian border lands (8), it is not safe to hazard any inferences as to the route by which this specialized ware, known between 2500 and 1000 B.C., as shown by Frankfort (9), throughout Western Asia and Crete, came into India or as to the people who introduced it. But if future research in Malwa (Central India), where 35 sites await systematic explorations, produce more tangible results, then the similarity between this specialized ware of India and Iran will acquire greater significance, and we shall have to consider how and when this Channelled-spout technique migrated to India from Iran (10) or Western Asia, which in our present state of knowledge seems to be its source.

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4. For instance Alaca Huyuk, pl. XLII, 1, and Fig. 192, 10 ; Chypre, pl. LII, 34 ; Lapithos and Vounous-Bellapais (Chypre), Figs. 196, 15-16, and 201, 12 ; Ras Shamra-Ugarit, Fig. 47, H ; Tarse (Asie Mineure), Fig. 175, 1 ; Alisar Huyuk (Niveau II), Fig. 193, 7, 27 in Schaeffer, Claude F. A., *Stratigraphie Comparée et Chronologie* (London, 1948).
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8. Though there are some definite similarities in designs between the painted pottery from Nāvḍā Ṭoli and sites in Makran, no cut-away spouted vessels are reported by Stein in his *Innermost Asia* (Oxford, 1928), 'An Archaeological Tour in Gedrosia, *MASI*, No. 43 (Calcutta, 1931), and 'An Archaeological Tour in Waziristan and Northern Baluchistan, *MASI*, No. 37 (Calcutta, 1929).



## NOTES AND NEWS

9. FRANKFORT, *op. cit.*, Table III. This table illustrates vessels which are truly beak-spouted, though the spouts are cut. True channel-spouts are shown in our illustration. These are done by my pupil and assistant, Shaikh Z. D. Ansari.
10. It may be here pointed out that Childe, Gordon V. ('Megaliths' in *Ancient India*, No. 4 (1947-8), p. 10), had noted the occurrence of porthole slabs in the Necropolis B at Sialk, and said, 'Yet Sialk B might be used to link with . . . the celebrated Indian dolmens . . . But they are concentrated in the south of the Peninsula in areas not likely to be affected by land-borne impulses from Iran'. Now, in this connection it may be pointed out that Nāvdā Toli has also yielded a black and red pottery of the S. Indian fabric and types in its bowls and dishes. So there are chances that Nāvdā Toli may provide a link between the S. Indian megaliths and Iran.

### THE MOSQUES OF ISLANG ISLAND, NEAR KHARTOUM (PLATE VIII)

[We are glad to publish the following note which corrects an error. ED.]

In his article 'Field Archaeology of the Middle Nile Region<sup>1</sup>', Dr O. G. S. Crawford identified a mud brick mosque on Islang Island, about 20 miles north of Khartoum, as that recorded in the *Tabaqat* of Wad Deifallah<sup>2</sup> as having been built by Agib el Mangilak for Hammad el Negid. As both Agib and Hammad were killed in 1611 at the battle of Karkog, a firm identification of this mosque would make it, apart from the converted church at Old Dongola, the oldest dated mosque in the Sudan, where few earlier than mid-19th century survive.

In the Annual Report of the Sudan Antiquities Service for 1952-53<sup>3</sup> doubt was thrown on this identification since local story claimed that the mosque was that of Fiki el Amin, a 19th century holy man. Further investigation has shown that this view is correct, and has discovered two other mosques on the island, both of some interest, and the site of Agib's mosque.

The mosque of Fiki el Amin, though named after him, was built by his son Abdullah on the site of one built by his father. The unfinished brick mosque which stands beside it<sup>4</sup> was built by Abdullah's son Hassan el Bisni, who died in 1946. El Amin's mosque must, therefore, be dated somewhere in the latter part of the 19th century. A plan is given in FIG. 1.

At the north end of the island are two ruined mosques of red brick. The more southerly of the two (PLATE VIII, A) is named after Abd el Karim Wad Agib, who is mentioned in the *Tabaqat*<sup>5</sup>. It was, however, built in the middle of the 19th century by one El Rayah Beshiri Wad Ahmed. For plan see FIG. 2. The other mosque, which is at the northern tip of the island (see FIG. 3 and PLATE VIII, B) is known as that of Wad Anis, but was also built in the 19th century by Mohammed Wad Khagali.

The story of Agib el Mangilak's mosque is well known to the people of the island, and the comment that they did not know of it<sup>6</sup> is incorrect. They say that this mosque was built at the north end of the island and immediately to the east of the mosque of Wad Anis. After it fell into ruins, it was rebuilt by Wad Anis, whose date is obscure. On

<sup>1</sup> *Kush* I, p. 25 and Plate x.

<sup>2</sup> For English translation of the relevant part, see MacMichael 'History of the Arabs in the Sudan', vol. II, p. 243, section 126.

<sup>3</sup> p. 2.

<sup>4</sup> Crawford, *loc. cit.*

<sup>5</sup> MacMichael, *op. cit.*, vol. II, p. 222.

<sup>6</sup> Annual Report, 1952-53, p. 2.

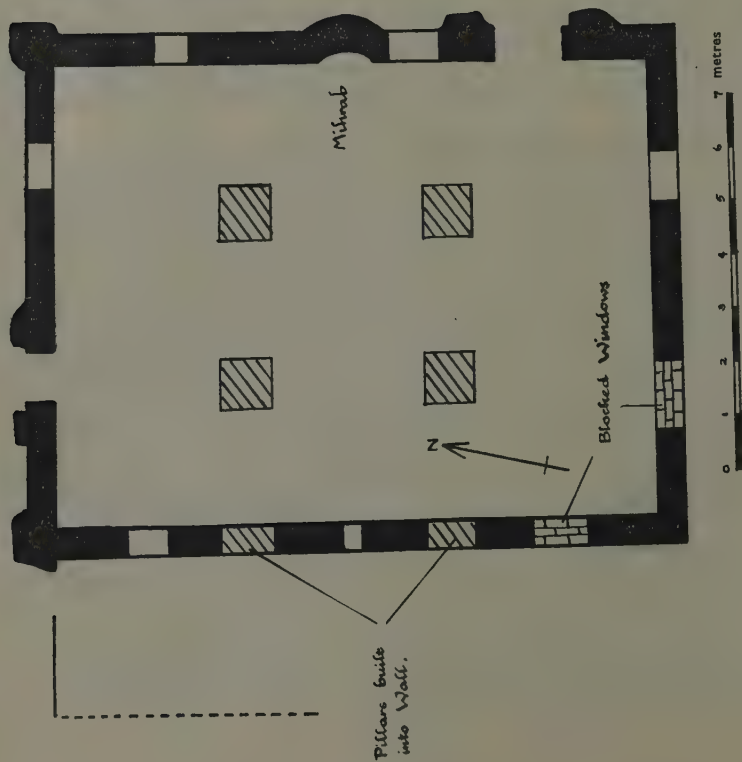


FIG. 1. MOSQUE OF FIKI EL AMIN

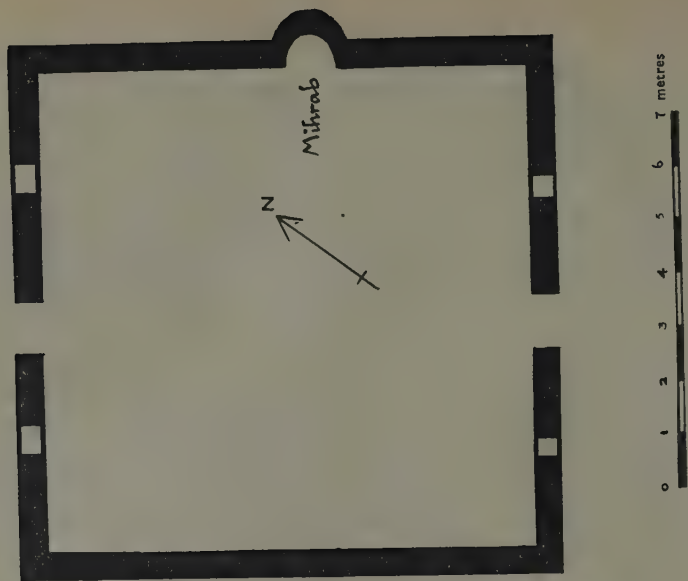


FIG. 2. MOSQUE OF ABD EL KARIM

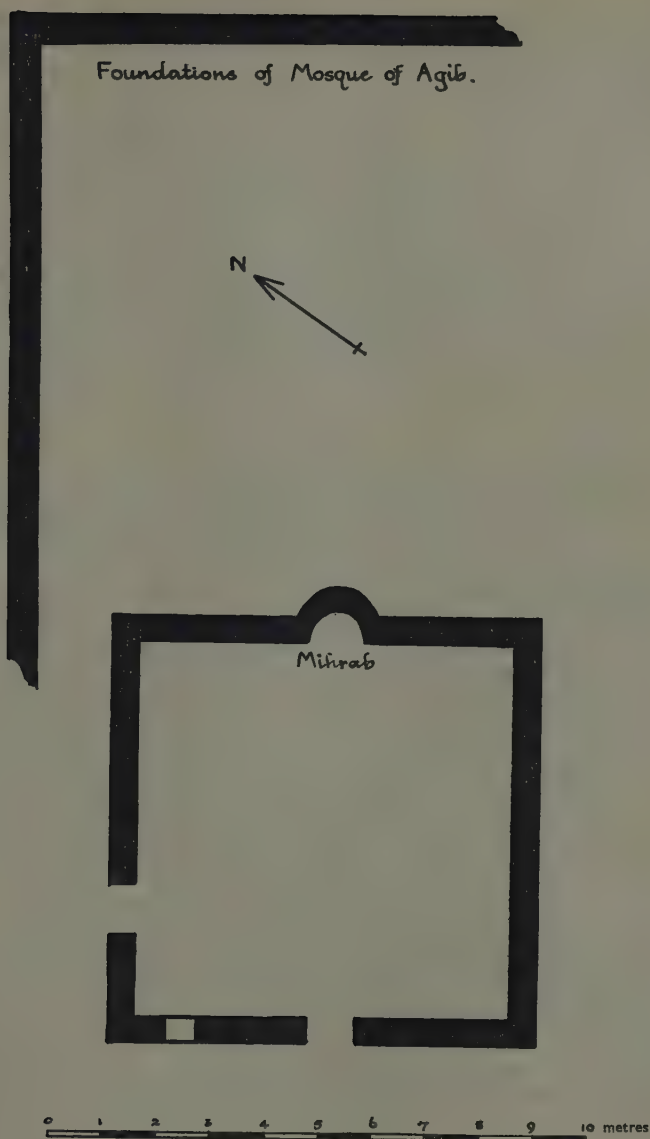


FIG. 3. MOSQUE OF WAD ANIS



collapsing once more, it was rebuilt as the present Wad Anis mosque, since most of the ground on which it had been originally built had been eroded by the river.

All that remains of Agib's mosque now is part of the foundation course of a once rectangular red brick building, as shown in FIG. 3.

The interest of the two northern mosques is the size of the bricks from which they were made. Many of these are extremely large, measuring 42 cm. × 20 cm. × 13 cm. The mosque of Abd el Karim is entirely built of these bricks and there are many in that of Wad Anis.

These bricks are quite different from those made in the 19th century, but are known from Christian times at Soba and in the Fung *gubbas*<sup>7</sup> at Arbagi. It seems likely that they are the bricks of the original mosque built by Agib, reused in later times. The normal local building material is mud brick, as in the mosque of Fiki el Amin, and it is unlikely that well-fired bricks of this unusual size would have been made during the period of the Turkia when these other mosques were built.

P. L. SHINNIE.

NIGM ED DIN MOHAMMED.

## USELESS DEVICES

Long ago it was suggested (by R. G. Collingwood) that the Foss Way, the longest direct stretch of Roman road in England, was made for a purely military purpose, and that afterwards it was not kept up and became derelict. An interesting modern parallel—up to a point—comes from China. In his book *Tibetan Marches* (Hart-Davis\*, 188s.), Dr André Migot, who travelled on it, says :—

‘ During the Second World War, for strategic reasons, the Chinese built what was meant to be a motor-road between Chengtu and Kangting; thence it was to run on, via Kantze and Jyekundo, as far as Sining. On this project were squandered not only millions of dollars, but the lives of hundreds—perhaps thousands—of Tibetans who were impressed for a task whose completion would bring no benefit to their people. The road was badly built; most of the money raised, by means of crushing local taxes, for its construction finished up in the pockets of contractors and officials. But at last it was formally opened in great style, with speeches, banquets, fireworks and military parades. After this a convey of three motor vehicles set off amid the plaudits of the multitude . . . The convoy did not get very far. One of the vehicles struggled on to Taofu, the luckiest of the three got beyond Kantze before it met its Waterloo, but none of them reached Jyekundo; the last part of the highway remains unsullied by wheels ’.

‘ After this inspiring effort, the highway was left to stew in its own juice. The reader may remember my description of the motor-road from Ya-an to Kangting, which is now little better than a mule-track. This Tibetan Autobahn suffered much the same fate. Rains, landslides, and the irrepressible vegetation eroded or invaded its surface; it went native again. No cars now attempt to use it, and caravans still follow the tracks which they have followed down the ages. Occasionally and always coincidentally, these tracks overlap the trace of the motor-road, so that for a time one ambles, feeling slightly bewildered, along a stretch of metalled road neatly subdivided by milestones; but for the rest of its length the project serves no useful purpose except to remind people of the follies of the Kuomintang ’.

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<sup>7</sup> Domed tombs of holy men.

\* To whom we are indebted for permission to quote, which we gratefully acknowledge.

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Here as in Roman Britain—if Collingwood's view is correct—a road constructed for a military purpose was soon abandoned. In China traffic followed the old caravan-track, which in the course of ages had surely found the best route ; in England it reverted to the Jurassic Way which, as the pre-Conquest charters prove, was in full use in the Cotswolds in the post-Roman period.

There are, of course, obvious dissimilarities. The Chinese road was unused because of the incompetence of its makers, whereas the Foss Way was obviously usable and may well have served its original purpose before it was abandoned.

Reflecting over such problems one is led to the consideration of another : How far, if at all, have defensive military constructions such as earthworks, walls and forts justified the labour of making them ? It is a startling fact that throughout history the majority of such constructions were never used at all nor was their efficacy tested in battle ; and that the majority of those few which were so tested proved useless. Modern instances will come to mind at once, and being within our own experience they are more convincing. The outstanding example of military folly masquerading (under the usual cloak of secrecy) as military necessity was the French Maginot Line, which was both pierced and out-flanked. In 1945 the German West Wall was no more effective than the Maginot Line had been. Older readers will recall the consternation when the vaunted Belgian fortresses failed to stop the German advance in the First World War. Equally useless in the end were all those elaborate Roman frontier-defences—the Roman walls in Britain and Germany and the walls and forts of North Africa and Syria. The barbarians trickled in just the same, usually from another direction ; the Saxons entered Britain from the sea, and were not stopped by the forts of the Saxon shore (Richborough, Reculver and so on) ; and it was the Arabs, not the Parthians, who eventually invaded Syria and Palestine, and took over the East and South.

Of all military works the most useless surely have been the forts built to protect a coastline against invaders ; in England we have examples from the Middle Ages to the 19th century, including the castles of Henry VIII (Netley, Hurst, Calshot, and those in Kent) and the anti-Napoleonic block-houses. It may be claimed that these and some other military works acted as deterrents, and so achieved their purpose bloodlessly ; that is a proposition which it would be difficult to prove or disprove. As a protection against invasion I prefer the English Channel. During the last war the Allies penetrated the defences of the Norman coast, and they were only able to do so because they had command of the Channel.

When we come to look at earlier or even prehistoric defences we find much the same sort of thing. Neither Maiden Castle nor Stanwick could protect their defenders from the Roman attack, and there is no evidence that the earlier (Iron Age A) hill-forts were ever attacked at all.

Thus we may suspect, on historical evidence, that most of the defensive remains of prehistoric times were either useless or unused, begotten by human folly out of original sin.

O.G.S.C.

## Reviews

STILL DIGGING. *By* SIR MORTIMER WHEELER. *Michael Joseph, 15s.*

This book is an autobiography. Readers of *ANTIQUITY* will know one of the characters there portrayed—Wheeler the archaeologist, who is the same as Wheeler the writer of the ideal *ANTIQUITY* article. They may also know him as the author of excavation reports (such as Bindon Hill and Stanwick) which are models of what such reports should be; and they will have heard that he was recently elected President of the Society of Antiquaries of London. But they may not know the other characters that this book reveals—the soldier, the administrator and (in its best sense) the adventurer and pioneer. They may be forgiven for their ignorance for they share it with many others. I don't propose to spoil their enjoyment of this book by enlightening them on all these points, but rather to make a few comments on it. I must say, first, however, that the book is brilliantly composed and is itself of more merit as literature than much that is applauded as such today.

Wheeler's outstanding achievement is to have improved out of all recognition the art and technique of excavation; and that covers everything from the initial planning to the substantive publication of results. His achievement is of course recognized by his colleagues and pupils, but one wonders whether it is really appreciated in academic circles and abroad? It is no slight achievement; it means that he has so improved our methods of recovering prehistory that he can settle on a site, apply his method and obtain a positive result. One hopes that his pupils may be able to do likewise; the signs so far are favourable. Wheeler's work in this sphere is, as he himself is the first to admit, a delayed follow-up of General Pitt-Rivers's excavations in Cranborne Chase which, carried out between 1880 and 1900, 'had brought archaeological digging and recording to a remarkable degree of perfection'. Although he had 'presented his methods and results meticulously in several imposing volumes . . . nobody paid the slightest attention to the old man'. Wheeler, as President, naturally cannot now criticize the Society of Antiquaries of London whose pre-Wheeler excavation record is bad, as exemplified in the post-Pitt-Rivers digs at Silchester, Old Sarum and Stonehenge. This society had inherited a thoroughly bad tradition from the days when it was dominated by collectors—the curse of archaeology—and medievalists. As a Society it has, up to now, hardly ever given a lead, though under the Presidency of Sir Hercules Read it did for a short time do a little better. It might at least have followed up the lead of Pitt-Rivers, but it didn't. That was a task that had to be undertaken by a later generation, decimated, or rather blotted out, as Wheeler says, by the First German War; and it was not shirked. It is only fair to the Society to say that later it aided and assumed the responsibility for some most important excavations such as Richborough and Maiden Castle. My criticism is that this aid was then already long overdue.

There are of course as many different ways of excavating as of writing tribal lays (though not by any means 'every single one of them is right'). For example, there are one-man digs such as Dr Stone's on Salisbury Plain which have been carried out impeccably and have added to knowledge by solving particular problems. There are slightly larger scale operations like those of Professor Piggott and Mr C. W. Phillips carried out on Long Barrows. And there are the well-known excavations of the Northern School on Roman sites. Sir Mortimer Wheeler's chief contribution to the technique of



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excavation has consisted in what may be called generalship—the planning of the campaign beforehand, the allocation of tasks, the selection of key-points on the site, and *pari passu* the training of his staff and the inspiration of their work. But inevitably he has at the same time developed innumerable technical devices which are now standard. This practical ability found full scope during his crowded four years as Director-General of Archaeology in India. One gathers from the book that he was as successful in training and inspiring his Indian pupils as he has been in England, but that they have to contend with disabilities altogether different from ours.

One of the best chapters in the book is that (Chapter 5) in which the author describes the foundation of the Institute of Archaeology which is now a department of the University of London. The need for some such centre was, in the twenties, very obvious, so much so that a similar idea occurred to me quite independently about the same time. Both Wheeler and I were then conscious of a mission and we naturally pooled our ideas. But anyone can have bright ideas, and it was Wheeler and Tessa who carried out the far more difficult task of translating the scheme from paper to bricks, mortar and people. It had in fact already been drafted in black and white at Cardiff in the winter of 1925-6, and from that time forward only minor alterations were made in the scheme. The idea had shaped itself in my mind as a sort of School of British Archaeology similar to the existing schools in Rome and Athens, and it was under this name that I have a note recording a discussion of it with the Wheelers in October 1927. They were then in London and actively engaged in promoting it, and I naturally left it entirely in their capable hands, though I continued to watch its progress with great interest and to ask for the latest news of it whenever we met. I did not in fact realize until I read this book how much further they had already developed the scheme on paper at a time when it was merely an idea in my mind.

Nearly one third of the book is devoted to the two War Interludes. Both are records of achievement that would do credit to a professional soldier, and in fact during the second Wheeler rose to the rank of Brigadier in command of artillery. A story is told about the Butte de Warlencourt on the Bapaume ridge where (though he does not mention the fact) he earned a Military Cross for a very heroic adventure. It is of no consequence at all, but the facts narrated on pp. 47-8 are a little out of focus, perhaps because condensed. My interest in the Butte began long before the Battle of the Somme and was, so to speak, purely academic. I was strolling round the draughtsmen's room one day in 1915 at 3rd Army Maps when I noticed that the draughtsman drawing the large scale map of that region had misinterpreted the air-photograph—an easy thing to do—and shown the Butte as a pit, not a mound. (It is probably a castle mound.) I was telling the story at the club (not at the Ordnance Survey) one evening as an example—not unique—of how even archaeological knowledge is sometimes of practical use. The Butte was a strong point in the subsequent battle, and it would have been rather disastrous if the maps had shown it as a pit. Wheeler was one of the party and it was because I saw he 'knew something' that I got him to tell us the story of his adventure.

But I have already exceeded the limits of space that would have been allowed to another reviewer, and must resist the temptation to say more about this fascinating book. A few concluding remarks about the book as a whole. The writer of the blurb, describes the author's life as having been spent in 'a period of remarkable transition in the history of archaeological craftsmanship'. He does not add, as he might, 'et cuius pars magna fuit', and one could hardly expect the author to do it for him. The discerning reader of the book could hardly fail to see that the author had a lot to do with the transition and of course his colleagues know it from first-hand observation. The general public

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probably still regards 'digging' statically; it is what archaeologists do, and the fact that the art has been developed and is still developing is, I suspect, hidden from them, as it obviously is from many who conduct excavations in other countries. It is not realized that British archaeology has shot ahead of the archaeology of all other lands, particularly in the art of excavation. This book explains how it happened. O.G.S.C.

A SHORT HISTORY OF THE SUDAN (Up to A.D. 1500). By MARGARET SHINNIE.

*Published by the Sudan Antiquities Service, Museum Pamphlet No. 1. pp. 8.*

*Price, 20 m/ms. (?= 6d).*

Many visitors to the Museum of Antiquities in Khartoum will be grateful to Mrs Shinnie for this pamphlet. In 6 pages, packed with matter, it tells the history of the Nilotic provinces during the prehistoric and historic periods to which nearly all the exhibits belong; a seventh page is devoted to Darfur. With no more space at her disposal it cannot have been easy to decide what to describe and what to omit, but I have little fault to find with Mrs Shinnie's selection.

I should have liked a few more lines on Reisner's discoveries at Kerma, the more so because the suggestion that the place was only a large trading post is mentioned. Is it conceivable that the tomb of Hepzefa with its numerous human sacrifices and the others with even more, 400 is the estimate in one tumulus, are only the tombs of successful traders? And the difference of the Kerma objects from those in contemporary Egypt might have been noted.

On p. 5 it is a shock to read about the mounds at Meroë that 'archaeologists had had a suspicion for some few years that these might represent the town' when Garstang excavated them. It would be truer to say that archaeologists had had no doubt about the fact for nearly a century.

I am glad that Mrs Shinnie has devoted a page to the Christian period, but the Nubian rule of matrilineal succession might have been mentioned: it may have done more than the 'almost incessant warfare' (p. 6) to establish Arab rulers.

A misprint, 2000 for 200, should be noted on p. 3, line 14. J. W. CROWFOOT.

EXCAVATIONS AT OLYNTHUS. Part XIV. Terracottas, Lamps and Coins found in 1934 and 1938. By DAVID M. ROBINSON, xx+534 pages, and 174 plates. *John Hopkins University Studies in Archaeology*, no. 39, Baltimore, 1952.

This is the last of the volumes devoted by Prof. Robinson to the excavations at Olynthus which he conducted in four campaigns between 1928 and 1938. It is hardly unfair to the excavator and his work to say that the most important thing that has happened to the city was its destruction by Philip of Macedon in 438 B.C. Indeed this event, which has given a valuable dating-point for the mass of cheaper and therefore most common artifacts of which Olynthus seems to have had more than its fair share, has afforded some of the most interesting results of the excavation, otherwise notable mainly for the plans of classical house architecture recovered there. Of the small finds of the last two campaigns presented in this volume the terracottas illustrate Olynthian taste from the 6th to the 4th centuries. A survey of terracottas found in Thrace and Macedonia serves as comparative and contrasting material to the number and types of the Olynthian finds described in the catalogue. The city appears to have been an active though unoriginal centre of terracotta manufacture; familiar Rhodian figures are imitated including protomai and seated and standing women, Attic and Boeotian comic actor types are copied, and it is Boeotia which is probably the intermediary for the strong Rhodian influence noted in the other figures

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(see Higgins, *Catalogue of Terracottas in the British Museum*, 289). Most instructive is the analysis of their find-places, particularly from a site whose houses and cemeteries have both been explored. Less than one quarter of the excavated terracottas come from graves, and only one deposit of early protomai illustrates their use as votives; on the other hand the large numbers recovered from the houses show how common they must have been as 'mantelpiece ornaments' and bric-à-brac in Olynthian, and no doubt most Greek houses.

The lamps are classified by the system adopted in *Olynthus* Vol. v which suits the material well, but the classical archaeologist still lacks any system of lamp classification which can serve adequately more than one site, let alone all Greek sites. The lamps themselves would have been better presented in fewer plates and drawings and more sections. With the coins is published a group of moulded flans prepared for striking. Appended is a Master Concordance of find-places by house, room and grave of all objects from all campaigns. This is invaluable, and the objection that their relations both with each other and with the floors of the rooms in which they were found are not recorded is not serious in a site destroyed as neatly as was Pompeii and in which any complete object in a room may reasonably be presumed to have been in use at the time of the destruction. The few misprints and incorrect cross references are ones which can be rectified by study in detail; the half-tone plates are good although the use of cut-outs in their composition is regrettable. Prof. Robinson is to be congratulated on the completion of the 14-volume publication, a task which is after all more exacting than the digging and ultimately the more important.

JOHN BOARDMAN.

THOMAS YOUNG: *NATURAL PHILOSOPHER 1773-1829*. By ALEXANDER WOOD. *Cambridge, at the University Press*, 1954. 30s.

'Phaenomenon Young' as he was called by his undergraduate contemporaries at Emmanuel College, half in derision, half in respect—after all he had been elected a F.R.S. at the age of 21, the year before he went up to Cambridge—began his career as a medical practitioner. He had been trained in London, Edinburgh and Göttingen as well as at Cambridge, and he practised at London and Worthing before becoming Professor at the Royal Institution. Here his work was on the undulatory theory of light and on the eye; he has been described as 'the father of physiological optics'. He subsequently held a variety of appointments including that of Inspector of Calculations to the Palladium and Eagle Insurance Companies, and Superintendent of the *Nautical Almanack* and Secretary of the Board of Longitude. His industry was great and his interests and abilities wide, as may be judged from the fact that he contributed articles of an authoritative nature to the *Encyclopædia Britannica* on such varied topics as Egypt, Refraction, Life Preservers, Carpentry, Chromatics, Cohesion, Roadmaking, and Porson.

To archaeologists he is of course for ever associated with the Rosetta Stone and the decipherment of Hieroglyphs, and of what he called 'enchorial', and Champollion 'demotic', script. The memorial tablet to him in Westminster Abbey describes him as 'the first' who 'penetrated the obscurity which had veiled for ages the hieroglyphics of Egypt', but there was, in his life-time, and until the present day, violent controversy as to the relative rôles of Young and Champollion in the decipherment. This book traces with fairness and impartiality the complicated story of the attempts at decipherment, and assigns true rôles to the work of de Sacy, Åkerblad, Champollion and Thomas Young, concluding as follows: 'No one who reads the evidence will suggest that Young rivalled Champollion as a scholar of Egyptology, or that his work in his famous article compares with the later contributions of that illustrious French savant. What seems to



me indisputable is that he led the way to the decipherment of the Egyptian hieroglyphs at a time when Champollion was right off the track'.

Young was himself encyclopedic, and this book succeeds in doing justice to his encyclopedic knowledge, and in giving us a vivid picture of this kind, generous, learned and versatile scholar. The first ten chapters are by Alexander Wood, who died before completing the book. He left notes on the two next chapters: these chapters and three more are the work of Frank Oldham, who had already published a short life of Young in 1933. The book also contains a short but characteristically clear and vivid memoir of Alexander Wood from the pen of Professor Charles E. Raven. Oldham shares Wood's admiration for Young and has his ability to handle in a scholarly manner the very varied facets of Young's work. To quote Oldham 'Young forms a fascinating subject in the field of biography, not only from his amazing scientific record and his wide classical learning combined with his remarkable depth of knowledge in philosophy, but also as a humanist working disinterestedly in the cause of truth'.

GLYN E. DANIEL.

**ROMAN IMPERIAL MONEY.** By MICHAEL GRANT. *Nelson*, 1954, pp. 324, 88 illustrations in text, frontispiece and 40 plates. Price, £2 10s od.

Professor Grant is in the van of researchers on the coinage of ancient Rome, and a wider public will be grateful for his latest book, incorporating much of his original and characteristic contribution to the subject. Coins are no longer to be regarded as a mere embellishment for the historian's footnotes, but are properly regarded as being (in Grant's words) 'a primary source of information about the Roman Empire'. His book will do much to recommend this point of view to those as yet unconverted. The evidence of what is depicted and inscribed on coins is as official and authoritative as, for instance, Augustus' *Res Gestae*—one of the earliest sentences of which (*rem publicam . . . in libertatem vindicavi*) seem but the echo of the remarkable inscription on a coin issued at the time of the so-called 'restoration of the republic', *Libertatis P(opuli) R(omani) Vindex*. Even when the Roman Government's 'official' view is mere misleading propaganda, it is still vital for the historian to know what it is, and it is often enough only the coins, among the surviving evidence, which give it.

The plan of this book is to present selected aspects of the Roman coinage, each illustrated from a different epoch. The reign of Augustus gets the fullest coverage, with an ample consideration both of the themes presented by the coins and also of the very complex organization of the monetary system (which the author considers to have been considerably more decentralized than is currently supposed, taking full account of the discoveries of actual dies and minting apparatus which produced the coins).

There follows an account of some purely provincial currencies of the early empire, of certain kinds of quasi-medallic coin, and of the problem of early imperial countermarks. A whole section is devoted to 'Personifications', including those of semi-divinized female personages of the imperial house as well as those more abstract concepts which attest the Roman aptitude for a complex and tortuous symbolism: the learned intricacy of some of these—such as the Pax-Nemesis figure who appears on some coins of Claudius, surely a brain-child of that erudite emperor himself?—reminds us how remote in many ways was the Roman mentality from our own. A further section on 'Anniversaries' reflects a line of interpretation which Grant has himself largely pioneered (though its length is somewhat disturbing to the balance of the book); references to anniversaries are occasionally explicit (as Philip I's '*Saeculum Novum*') but more often, if Grant is right, are delicately allusive, or exploit the lucky coincidence, e.g. when Vespasian, almost exactly a hundred years after Actium, copies one of Augustus' coin-types to point a

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parallel between Augustus' position and his own. The 'anniversary' motive may also, as Grant suggests, lie behind the emphatic inclusion of honours to Julius Caesar in the remarkable 'restored' coins of Trajan. And for any who may be sceptics about anniversaries, Grant has a very apt quotation from one of Cicero's letters to Atticus (p. 179: *Ad. Att.* IV, 1, 4).

A final section describes the gradual decline and fall of the monetary system originally developed by Augustus, successive debasements of the coinage reflecting the growing impoverishment of the Roman state, mainly (Grant considers, p. 249 f.) on account of the strains of war and the ever-increasing necessity for appeasing the armies with *donativa*.

From Grant's exposition, and from the illustrations, which are lavish and of high quality, the non-specialist readers should be well enabled to grasp the idiom of Roman coinage and its historical content: the book is handsomely produced, notably free of misprints, and the critical notes gathered at the end will also be useful to the specialist (though there are too many footnotes in the text). One cannot expect every feature of so vast a field to be charted, but it is perhaps to be regretted that the later period of the Empire—admittedly beset with many thorny and unresolved problems—is given less attention than the earlier (e.g. only one coin later than Diocletian is illustrated), if only because the less satisfactory nature of other sources for the later empire gives even greater value to the historical evidence of the coins: and greater emphasis on the later third and 4th centuries would have given a better perspective to some of the topics discussed. Thus, the 'world coinage' towards which Augustus took some tentative steps (ch. 3) only became a reality, strictly speaking, after Diocletian's reform of 296. Until then, the imperial coinage catered mostly for the western half of the empire, leaving the Greek eastern half with a varied array of currency, mostly supplied by the cities, and still inscribed in Greek (as the finds from excavations at such places as Tarsus and Dura-Europus make clear): only after 296 was an imperial coinage of uniform type, with Latin inscriptions throughout, supplied to all parts of the empire indifferently (from a large number of mints which are named on the coins). This final stage of unification, with the complete disappearance of all Greek and local currencies, is hardly given the importance it deserves as a factor of interest for the historian. G. K. JENKINS.

BULLETIN DE L'INSTITUT ARCHÉOLOGIQUE BULGARE, vols. XIV (1943), XV (1946) and XVI (1950).

FOUILLES ET RECHERCHES: 1. Travaux des Sections préhistorique et antique du Musée national bulgare (1948); 2. Apollonie pontique (1948).

LIFE AND CULTURE OF THE PRE-CLASSICAL AGE IN BULGARIA (1952). Sofia, dates as given.

These Bulgarian publications cover the period during and after the Second World War. The three volumes of the Bulletin, the third a well earned Festschrift in honour of Gavril Kazarow, fully maintain the standard set by that publication in the pre-War period. The two volumes of *Fouilles et Recherches* begin a new series for the publication of work carried out by the Prehistoric and Classical Departments of the National Museum. The first volume contains a number of reports covering the whole field; the second is devoted to Apollonia Pontica. The variation in format between these two volumes is irritating and an agreement to use the same format as the Bulletin would have had much to commend it. Foreign scholars will also regret that, unlike the Bulletin, these volumes are not provided with French or German captions to the plates in addition to the



summaries at the end of each article. Technically the half tone illustrations constitute the weak point of all these publications. The photography, both of sites and of objects is generally poor—often out of focus—and the reproduction of numbers of small pictures set among the text on unsuitable paper does nothing to enhance the results. The volumes are badly bound with the fascicules set independently in a paper cover to which they are lightly gummed, a method unsuitable for substantial volumes of a large 4to size.

*Life and Culture of the pre-classical age in Bulgaria* is a popular guide to the prehistoric collections of the National Museum. The text is in Bulgarian with French captions beneath the 17 plates. The publication serves its purpose well and a French edition would be of value to western visitors. The last plate provides evidence of a thorough and apparently up-to-date rearrangement of the galleries.

The following notice calls attention to the more important results published in the five volumes of the two series, including those which bear on archaeology outside Bulgaria. In addition there are many notes and records of finds of lesser importance, which help to build up a fuller picture of life and culture in Bulgaria in the prehistoric, classical and medieval periods.

The principal articles on prehistoric research in Bulgaria are to be found in the first volume of *Fouilles et Recherches*. The late neolithic sites at Mikhalitch (near Svilengrad, on the Maritza above Adrianople) yielded pottery of a 'Trojan' type akin to that found in Thrace and Macedonia. The report mentions a fortified city with the foundations of rectangular houses formed of plaited branches plastered with clay, but gives no plan. Aeneolithic settlements with the normal Bulgarian graphite wares are recorded at Krivodol, Kolena and Tchelopetch. The first site, near Vratza in north-west Bulgaria, is important for the association of graphite ware with ochre painted pottery, the ochre applied directly to the smooth surface of the vase. This technique, not previously recorded in Bulgaria, resembles the crusted wares found further west in Jugoslavia.

A chance discovery of a Thracian tomb near Assenovgrad in South Bulgaria produced a typical helmet together with a pair of greaves stamped in Greek with the name 'Ἀγαθάνωρ'; this is attributed on epigraphic grounds to a date at the end of the 5th or in the first half of the 4th century B.C. and gives a secure chronological link for this type of helmet. The article discusses other finds of this armour in Bulgaria. The list of Bulgarian waggon burials of this age with illustrations of the objects is a useful compilation (*Bulletin* XIV). A Hallstatt cemetery of the normal Macedonian type is recorded from Rudanza, north of Ishtip in Yugoslav Macedonia.

The most important classical report is the volume of *Fouilles et Recherches* dedicated to Apollonia Pontica. There were 121 graves dating from the 5th to the 3rd century B.C. excavated in the cemetery. Six inscribed stelai of c. 400 B.C. were found among the graves. Four others are also published from the immediate neighbourhood of the cemetery, together with four further inscriptions from the modern town, which covers the site of the Greek city. The last include a decree of the Boule of Mesembria thanking Antiochus II for help against the Celts and a milestone of Vespasian. The finds—pottery and figurines—are fully published. The earlier graves at Apollonia are those of the Greek colonists, but the excavators note that the more recent show strong native, i.e. Thracian, influence. There is a good account of the Odeion at Nicopolis ad Istrum, illustrated with reconstructed drawings (*Fouilles*, I). From Sofia there are notices of a temple of Serapis and a painted Christian hypogeum (*Bulletin* XIV). A list of 176 inscriptions from Varna (Odessos)—many unpublished—is a useful compilation with good illustrations (*Bulletin* XIV). A diploma of A.D. 82 found in 1944 at Kamensko, near Razgrad refers to Roman troops in Germania Inferior (*Bulletin* XV). An inscription of the 4th century A.D.



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from Bech Tepe in Yugoslav Macedonia is of interest as it refers to the building of a gate at Bargala, which may be identified with the ruins on that site (*Fouilles* 1). Finally reference should be made to Vulpe's account (*Bulletin* xvi) of the Lower Bessarabian limes more fully discussed by me elsewhere (*ANTIQUITY* xxviii, 1954, 224-6).

Three long and interesting reports on Pliska (*Bulletin* xiv) carry the story of this early Bulgar capital beyond the point recorded in this Journal (*ANTIQUITY* xiii, 293-303). The most interesting of the new discoveries is the evidence for an earlier and larger building beneath the great palace of Omurtag (plan: *ibid.*, fig. 3). Only the foundations of this earlier building survived, forming a great checkerboard of 63 roofed chambers. This, it is convincingly argued, was the podium of the palace erected by Khan Krum, the palace itself lying on the upper floor. Krum's palace was burnt in 811 and the quantity of charred wood and ashes discovered in the foundation trenches show that it was largely constructed of timber, on a stone basis. The architectural reconstruction is based on later Byzantine parallels and the Tekfar Serai at Constantinople is illustrated as a surviving example of more recent date. Pliska, as both palace and other buildings show, was a great nomadic encampment translated into permanent form. It carried on the tradition of Attila's camp and it is only to be expected that the authors should find a parallel between their reconstruction of the palace and Priscus' account of the dwelling of the Hunnish king. An inscription from Preslav is of interest as it is written in a language of the Turkish family illustrating the speech of the Bulgars before their adoption of a Slavonic dialect.

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THE DOMESDAY GEOGRAPHY OF MIDLAND ENGLAND. By H. C. DARBY and I. B. TERRETT. *Cambridge University Press*, 1954. Price, 55 shillings.

The second volume of Professor Darby's important work has been awaited with some excitement. The first volume so far exceeded expectation in the beauty of its format and the generous scale of its illustrations that it seemed hardly possible that the standard could be maintained. Nevertheless, Professor Darby and Mr Terrett, with the aid of the Cambridge University Press have produced a second volume which, like its predecessor, is a model piece of book production. The first volume contained 109 maps. The present volume contains 159. It is perhaps a little disconcerting to find the Welsh border counties of Hereford and Salop and Gloucestershire included in Midland England (more usually called the Midlands) and the counties of Nottingham and Derby excluded, but the necessity of gathering all the Domesday accounts into the six volumes must have dictated the distribution of the shires within each volume of the work. The present volume deals with Gloucestershire, Herefordshire, Shropshire, Staffordshire, Worcestershire, Warwickshire, Leicestershire, Rutland, and Northamptonshire. Professor Darby himself wrote the chapter on Gloucestershire and contributed a concluding chapter summing up the findings of the previous chapters. Mr Terrett wrote the chapters on Northamptonshire and Rutland. Mr Atkins dealt with Herefordshire; Mr Sanders with Shropshire; Mr Wheatley with Staffordshire; Mr Monkhouse with Worcestershire; Mr Kinvig with Warwickshire; and Mr Holly with Leicestershire. This is an example of team-work so highly organized that there is little to distinguish the style of one author from another. Indeed, Professor Darby has clearly been insistent on the use of the same formulas, a wise insistence, but one which makes it more difficult to read straight through the book. These volumes are not intended for light reading. They are reference books to be jealously guarded and frequently used.

The editors have had to deal in the present volume with many instances of changing county boundaries. Domesday Worcestershire, for example, had outliers which have been

transferred to no less than five different counties. The modern Worcestershire has taken in villages which lay in four Domesday counties. It is a right decision for geographers in mapping the survey to adjust the work to the modern county boundaries, but it makes the historian pause for a moment to adjust his own outlook. He then realizes how useful in other contexts these neat indications of changed boundaries will be.

The inclusion in one volume of counties so diverse as those of the Welsh border and central England gives a peculiar interest to the present volume. It is a foretaste of the birdseye view of English economic and social conditions which the final summing up will afford. In view of the wide divergence in the number of slaves between, say, Gloucestershire and Rutland it is of some importance to consider how nearly uniform were the questions put to the jurors in different parts of England. No absolute assurance on this point can be felt, but the survey was planned as a single operation. It would therefore seem very likely that the questions were carefully drafted, just as the articles of the eyre were carefully drafted by Hubert Walter, archbishop of Canterbury and Justiciar, over a hundred years later. The different picture which the answers to these questions give in different parts of England would therefore seem to be a genuine reflection of a different society.

The inevitable omissions from such a record as Domesday Book are tantalizing. The most obvious are churches, for there must have been many more than are recorded in any county. No map of Domesday churches would be worth drawing. There must surely have been more meadow in Herefordshire than Domesday Book can be made to show. There seems less woodland in that county than might be expected from the dense forests which in the 8th century made it unnecessary for Offa to draw the line of his dyke across the central plain of the shire. Incidentally it might have been better to draw a dotted line for that part of the dyke in the very interesting map showing the Welsh evidence provided by Domesday Book. Domesday Book is erratic in its recording of women, who would not normally be heads of households. Presumably the two widows of Besford, Salop, the nine female cottagers of Stokesay in the same county, and the one widow of Badsey in Worcestershire were heads of houses as a result of the deaths of their husbands. There must have been more than twelve widows in these nine shires in 1086. *Valentia*, pp. 164, 169, and 170, should have caused no difficulty. The passage in which it occurs is merely stating that the value—*valentia*—of certain berewicks is included under the chief manor of Lichfield.

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